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Abstract

We use geocoded administrative establishment data in Texas to estimate the effects of localization economies on the spatial persistence of industrial employment in the software industry. The choice of the software industry allows us to distinguish between the spatial persistence of employment due to human capital spillovers from that due to the labor pool channel. Unlike previous research, this analysis is independent of administrative boundaries. The results suggest that a location, defined as a 1-mile radius circle, with an initial concentration of software industry employment, retains a disproportionate number of employees 6 years later despite significant job turnover. Software industry employment in surrounding areas has small effects. The results are not driven by higher establishment growth rates in high concentration locations or by differences in survival probabilities. They are fully explained by: (i) the retention by other establishments in a location of jobs lost by an establishment in that location, and (ii) an increased propensity of software establishments to enter in or near locations with prior software establishment presence. The entry effect diminishes sharply beyond one mile. We demonstrate that these findings are most consistent with labor channel effects, although the presence of human capital spillovers cannot be fully excluded.

JEL Classification: R12.
Keywords: Agglomeration economies, labor pools, knowledge spillovers, firm growth.

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1 Introduction

The geographic distribution of high technology firms in the United States differs significantly from the distribution of broader economic activity. Silicon Valley in California, the Route 128 Corridor in Boston, the Research Triangle in North Carolina, and the greater Austin, Texas, region are well known examples of high technology industry concentrations. This phenomenon is, of course, not exclusive to these relatively new industries. Marshall postulated as far back as 1920 that the presence of specialized inputs and upstream service providers, labor pooling, and knowledge spillovers are localized and help to explain why certain industries that are not otherwise tied to geographically specific inputs are spatially concentrated. A voluminous empirical literature has since confirmed the ubiquity of spatial aggregation and investigated the effect of agglomeration spillovers on technical change, firm growth, productivity, and other outcomes. Moreover, spatial aggregation tends to be persistent over time because agglomeration generates self-sustaining dynamics through these positive spillovers.

In this paper, we identify the separate effects from a localized labor pool and knowledge spillovers on the spatial persistence of employment in the software publishing industry in Texas. To isolate these effects, we use panel data at a very high level of spatial and industrial resolution that enable us to achieve a novel approach to spatial sampling. We observe the variables of interest within localities of constant size (radii of our choice) that contain the requisite infrastructure and zoning for software publishing activity. Thus, we are able to control the spatial context to reach conclusions on the nature and geographic reach of agglomerating influences. We are not aware of any other study that utilizes this spatial sampling methodology.

The software publishing industry lends itself well to the investigation of labor market and knowledge spillovers. The industry's output is primarily intangible intellectual property that sells in the state-wide (if not national or global) market. Thus, locational factors such as access to natural resources, local demand, proximate input suppliers, and transportation costs are not particularly relevant. It is a relatively new industry, so historical factors should not have a significant impact on firm and industry location. Moreover, capital inputs are not typically fixed (beyond the period of the current building lease) or tend to depreciate very rapidly (e.g., computer hardware and software), so past investment decisions in physical plant do not constrain firm location over the medium term. On the other hand, given the highly dynamic and competitive nature of the industry, the rapid evolution of software development, and its heavy reliance on specialized human capital, software publishing seems well suited to benefit from labor pooling and knowledge spillovers.

Computer programmers, the most important labor input in this industry, tend to be young and mobile. Therefore, if labor market driven localization economies are generally present, their effects should be easy to detect. The absence of other plausible channels for such spillovers, and our ability to control for localized economic activity using data on other business establishments permit us to measure the effects of these labor channel localization economies.

Our starting point is the observation that geographic clusters of software establishments tend to be highly persistent. Within areas of 1-mile radius, we find that the elasticity of software employment at the end of 2006 with respect to employment at the start of 2000 is approximately $0.50 - 0.75$. These are very high numbers given that the majority of software jobs that were present at the start of 2000 were lost. In fact, at the establishment level, we find the elasticity of software employment to be in the $0.30 - 0.35$ range. Our results also indicate that end-of-period employment is lower for establishments located in areas with a prior presence of other software establishments, even given an absence of competition on the product side. Although this effect is rather weak, it is suggestive that the presence of other software establishments reduces rather than enhances the success and growth prospects of other incumbents. High entry rates should have led to further dilution of any initial location advantage, but they do not.

The difference between the firm-level and location-level elasticities, and the fact that firm level employment is decreasing in the industry activity in the same location, suggests that many of the jobs lost by a software firm in a location were captured by other software firms located no more than one mile away. This, however, is not sufficient to fully explain the high elasticity at the location level, leaving the location choices of new entrants as the likely source of the additional jobs. To confirm this possibility, we investigate entry rates at different locations and follow the survival of these entrants as a function of location characteristics (including prior presence of other software establishments). After controlling for localized presence in a control group of other businesses, we find that software firms tend to enter in locations that are within one mile of other software firms. We find that the attractiveness of a location diminishes very rapidly with distance and almost disappears after 5 miles. There are no comparable effects on exit at any distance threshold.

It is clear that the high spatial persistence of employment in the software industry is driven by two components: (i) the propensity of software establishments to locate in areas with pre-existing software establishments, and (ii) the ability of co-located firms to act as a "sponge" for jobs lost by their neighbors. The spatial persistence is not driven by differential growth rates of establishments located in areas with substantial software firm presence or by differential survival probabilities of those establishments.

Our analysis of this evidence leads us to conclude, given weak spatial specialization of software firms (as we demonstrate), that there is a substantial spatial effect operating via the medium of the labor market. Close proximity facilitates the transmission of information about employees of other firms and increases match value for prospective employers; it also reduces switching costs, as employees would not need to make adjustments in living and commuting arrangements. These factors reduce the set-up costs of establishments and subsequent recruiting costs. Thus, potential entrants tend to choose a location near other existing firms and departing employees of a firm tend to locate a spin-off in the same location. Our finding that other software firm presence in very close proximity tends to marginally decrease employment growth at the firm level, even though it seems to increase employment at the industry level (beyond what would be expected by mere employment inertia), is inconsistent, all else equal, with the presence of positive knowledge externalities. We recognize, however, that limited knowledge spillovers are not inconsistent with the presence of larger labor market effects and might help to explain why the increased entry in high agglomeration locations of some (presumably) marginal firms does not depress the average survival rate.

2 Conceptual Framework and Identification

2.1 Agglomeration Effects

Agglomeration economies have been cast in terms of urbanization economies as described by Henderson (1986) and Krugman (1991), and in terms of own-industry localization economies in the Marshallian vein. Relatedly, Glaeser *et al.* (1992) refer to the Jacobs-type urbanization externality that arises when industrial diversity enhances opportunities for inter-industry knowledge spillovers.[1] Agglomeration economies (including knowledge spillovers) have been examined from the perspectives of firm location choices (Rosenthal and Strange (2003), Woodward et al. (2006)), firm exits (Staber, 2001), industry growth (Glaeser et al. (1992), Henderson et al. (1995), and Combes (2000)), and labor productivity (Ciccone and Hall (1996)). Glaeser et al. (1992) and Henderson et al. (1995) distinguish between the concepts of static versus dynamic externalities. While the notion of a static agglomeration externality can explain the location of industries, industrial growth suggests a dynamic character to the externality. As articulated by Henderson et al. (1995), dynamic externalities arise as information and experience accumulate through time within the locality. As the concentration and urban density increases through time, there is a deepening in the specialized labor pool and input suppliers while labor mobility and matching between firms is enhanced. Glaeser

[1] Their reference on this is Jane Jacobs, The Economy of Cities, New York: Vintage 1969.

et al. (1992) introduce the term Marshall-Arrow-Romer externality to describe the dynamic localization economy. Combes (2000) notes that a greater number of similar firms in a locality increases the likelihood of knowledge spillovers since there is greater likelihood of closer matches between firms.

Since knowledge –as distinct from information– gleaned from experience tends to be embodied in the individual, worker mobility and direct personal and professional interaction are probably the primary channels by which un-priced knowledge spillovers are conveyed. Close geographic proximity should facilitate those channels. For example, workers who happen to meet each other at other venues can far more easily meet (e.g, for lunch, coffee, or even car-pooling) if their places of employment are close to each other. Being in the same business cluster, where one might even walk to a meeting, reduces the barriers for such meetings. Interactions are far harder to arrange if the location of employment is even five miles away. The net knowledge spillover is expected to be positive although there may be "winners" and "losers" for any specific transaction. When one firm absorbs an idea from another, it may well be to the detriment of the source firm if there is competition between the firms.[2]

The labor pool effect may in fact have several expressions. A larger pool of software industry workers within commuting distance allows for easier expansion of a firm through the poaching of employees of other firms, and also reduces the set-up costs for new entrants. The overall effect on firm productivity and growth could be positive if, as noted, employee-employer match quality improves by this practice. A large pool of employees may itself be an attractant for software engineers and programmers to move into the area, deepening both quality and diversity of skills in the locality. The labor channel can also operate through the founding of start-up firms by employees departing their current employer and creating another firm. These firms are often located very close to their "parent" firms, sometimes in the same building or block. As with knowledge spillovers, there is the likelihood that these localized effects have both winners and losers as some successful firms may benefit to the detriment of their neighbors. In fact, as one industry executive confided to us, programmers employed by different firms compare their work conditions and terms of employment when in social contact and are ready to switch employment when their current employer is not competitive.[3]

Consistent with both the labor pooling hypothesis Freedman (2008) finds evidence that spatial clustering facilitates localized worker mobility in the software publishing industry. Rosenthal and Strange (2003) report

[2]Though extremely unlikely, spillovers might conceivably even be negative in aggregate if they lead to free-riding. For example, firms may reduce experimentation with new ideas hoping to piggy back on ideas developed by other firms. On balance, this could possibly lead to a reduction of available knowledge.

[3]Some Silicon Valley firms run company buses from San Francisco to their facilities for the purpose of easing commuting. Company buses also reduce, incidentally or intentionally, employee interaction across firms (relative to car pooling, public transport, or other non-company transport), suggesting that such interaction is not valued by firms.

a quote from Saxenian (1994) in which a high-tech worker from Silicon Valley states, “The joke is that you can change jobs and not change parking lots.” For such localized job changes, search and transactions costs are probably negligible. This latter quote has implications for the appropriate geographical area over which the distinction between the Marshall-Arrow-Romer and Jacobs externalities can be observed. Indeed, Rosenthal and Strange (2003) and Wallsten (2001) note that localization externalities and knowledge spillovers attenuate rapidly within one mile.

While both knowledge spillovers and labor pool effects are likely to lead to a positive association between agglomeration and spatial persistence in the software industry, the effects are expected to differ subtly between these two sources of persistence. The question is whether industry growth and spatial persistence are driven by the growth of the existing establishments in a location or by the entry of new establishments and their survival.[4] Labor market effects may lead to the retention of jobs in a locale because the mere availability of labor attracts entry, even without improving the survival prospects of the new entrants or, for that matter, the success of the existing firms. Labor market effects can also improve firm growth and survival if thicker labor markets improve employee-firm matching and reduce ongoing costs of recruiting.

In the absence of any observed entry effects, increased growth and survival rates are unlikely to be driven by labor market effects. Rather, knowledge spillovers are more likely to lead to gains for existing firms and better survival prospects for entrants (given that software publishers compete in a national market). Of course, by increasing growth and survival prospects, knowledge spillovers would also increase entry. However, in the absence of any observed growth and survival effects, increased entry propensity is unlikely to be driven by knowledge spillovers. Labor market effects would be a more persuasive explanation in this case.

2.2 Conceptual Framework

Though this discussion does not constitute a formal model, the comparative statics we describe can be made concrete with some analysis and illustrated diagrammatically. Consider all potential establishments in a location l and rank them on the basis of their expected present value gross of entry costs. Let this present value be given by $V(p) = \theta(p) + \kappa z_l + \lambda\theta(p)z_l$, where p is the percentile ranking of an establishment, $\theta(\cdot)$ is an increasing function, z_l is a measure of value-enhancing spillovers in location l, and κ and λ are positive constants. Let the fixed costs of entry of any establishment be $FC = \gamma_0 + \gamma_1 x_l$, where x_l is a measure of the entry barriers in a location and γ_0 and γ_1 are constants.

[4]We note parenthetically that employment growth and exit are qualitatively different in that exit is a “tail event.” Thus, an increase in the variance of outcomes may not affect mean firm employment growth, while affecting survival probabilities.

For any location, l, the establishments with a gross present value that exceeds entry costs will enter and operate; the rest will stay out. The fraction of potential establishments that enter, i.e., the entry threshold percentile, is given by $\hat{p}_l = \theta^{-1}\left(\frac{\gamma_0+\gamma_1 x_l-\kappa z_l}{1+\lambda z_l}\right)$. This is decreasing in any value-enhancing spillovers and any location characteristic that reduces entry costs. In this framework spillovers do not necessarily increase the present value of all establishments, but they do shift the distribution of present value so that a distribution with higher first-order spillovers dominates a distribution with lower spillovers.[5] This framework is static, but dynamic implications are discussed below to better connect the framework with the data.

Figure 1, panel A, illustrates within this framework how entry and firm performance differ across two locations, one with low and the other with high knowledge spillovers. The horizontal axis plots the percentiles of potential entrants, and the vertical axis plots the entry costs and the gross present value. The upward sloping lines are the relationship between firm present value and establishment percentile rank for low and high spillover locations. For this figure, $\theta(\cdot)$ is assumed to be linear. The line for the high knowledge spillovers location is above the line for the low knowledge spillover location, indicating that knowledge spillovers are value enhancing, i.e., increase the value of z_l. The high z_l line is steeper, i.e., $\lambda > 0$, suggesting a stronger positive effect at the high end of value distribution. The fixed costs of entry are the same for high and low knowledge spillover areas, consistent with the premise that knowledge spillovers do not affect the set-up costs of the establishment. The marginal entrant in the low and high spillover location is $\hat{p}_{low}$ and $\hat{p}_{high}$, respectively. High spillover locations attract more firms. Moreover, the average performance (value) of these firms is higher. With the distribution of establishments over percentiles being uniform by construction, the mean expected value of the operating establishments is increasing in z_l.[6] The continuation value and (future) employment of an establishment evolves over time, but is positively correlated with its initial expected value. Thus, locations with high values of z_l are expected to have more and faster growing establishments. When an establishment's continuation value drops below zero, the establishment exits. With the distribution of establishment expected present values being higher in locations with high values of z_l, the exit rates in those locations should be lower.

Panel B of Figure 1 illustrates how entry and firm performance outcomes differ across two locations, one with low and the other with high labor market agglomeration economies. Agglomeration effects arising

[5] For simplicity, fixed costs are the same for every establishment, but an extension to a framework that relaxes this is discussed in footnote 7.

[6] $E[V(p)|p > \hat{p}_l] = (1+\lambda z_l)E[p|p > \hat{p}_l] + \kappa z_l$. Observing that $\hat{p}_l = (\gamma_0 + \gamma_1 x_l - \kappa z_l)/(1+\lambda z_l)$, and that by definition p is distributed uniformly, we obtain $E[p|p > \hat{p}_l] = 1/2 + (\gamma_0 + \gamma_1 x_l - \kappa z_l)/(2(1+\lambda z_l))$. Substituting in the expression for the establishment's expected value and simplifying, we get $E[V(p)|p > \hat{p}_l] = (1+\lambda z_l + \gamma_0 + \gamma_1 x_l + \kappa z_l)/2$ which is increasing in z_l.

from the labor channel reduce the cost of entry into a location. They can also reduce ongoing recruiting costs and thus shift up the distribution of gross present value of establishments. In the notation of the analytics, labor market agglomeration economies decrease x_l and increase z_l. The second effect is expected to be smaller. Whether or not this is the case, the important point is that the labor channel affects both set-up costs and also future performance. As can be seen from Panel B of Figure 1, average present value of entering establishments in locations with high labor agglomeration may be lower, even though they attract more establishments (as demonstrated in footnote 6, the expected value conditional on entry falls as x_l decreases, but rises with z_l). In fact, we believe that it is likely lower, since the entry effect is expected to dominate. But this will depend on the relative size of the shifts of the two lines and the extent to which $\theta(\cdot)$ departs from linearity (and in which way).[7] Following the discussion in the preceding paragraph, the expected value of an establishment affects its future employment level and exit probability. In markets with high labor agglomeration economies, then, establishment growth rates may be lower and exit probabilities higher than in locations with low agglomeration economies.

These two figures are both static representations and describe entry in a single period for a specific level of spatial agglomeration. Spatial agglomeration will persist over time but persistence will not be perfect, as random variation in firm outcomes will weaken the initial advantage of some locations and strengthen that of others (even if there is a systematic relationship between agglomeration, entry, survival and growth). Spatial persistence may be particularly high if the source of agglomeration economies is the labor pool channel. When a firm receives a negative shock and lays off workers, it creates a pool of local job seekers and reduces the recruiting costs of co-located firms, facilitating their expansion. This tends to stabilize employment at the location.

3 Spatial Sampling, Estimation Framework and Data

3.1 Location Definition

A key feature of our analysis is to identify the geographic scale at which agglomeration tendencies are relevant. If agglomeration effects dissipate rapidly over space, at one mile or less, observing geographies at the sub-county level is critical.[8] In principle, this would be accomplished by looking at county sub-divisions,

[7] Upon reflection, one can discern that heterogeneous entry costs do not alter any of this discussion as long as firms with high post-entry value enter "ahead" of firms with low post-entry value. A more substantial modification of this framework, under which firms have the same post-entry performance but differ only in the entry costs (represented by a flat post-entry cardinal performance curve and upward sloping entry cost curve), results in some meaningful changes in the comparative statics, but cannot be reconciled with the empirical results.

[8] Some of the variables discussed in sub-section 3.2 are by necessity defined at the county level.

such as census blocks, but these are of variable geographic extent, irregular shape, and are constrained by county boundaries. Ideally, we would like locations to be equally sized and defined in a purely spatial manner. Having the ability to precisely control the distances over which the analysis takes place is essential to measuring the range of these agglomerating effects.

Another desirable feature for the definition of locations is that they only include plausible destinations for software establishments. At small geographic scales, there are areas that do not offer the basic conditions for the entry of software publishers. Inclusion of areas with zero probability of containing a software establishment is problematic in two ways. First, it would not be consistent with specifications which implicitly assume a positive expected number software entrants i.e., it would require the use of models where for most locations an outcome of no entry happens with probability 1 regardless of covariate values. Second, it would result in a plethora of observations/locations, only a tiny fraction of which would experience software firm entry over our sample period. In defining locations, we adopt the principle that human capital requirements can always potentially be met, at some cost, at any location either because required employees are already available for hiring locally or because they could be induced to move into the area. However, the locations of firms are restricted by zoning laws, by the availability of suitable building stock, and by the presence of complementary infrastructure (e.g., roads, utilities). We posit that the likelihood that a software firm enters in a location where these conditions are not satisfied is zero.

Our approach to identifying potential locations is as follows. More than 90 percent of software establishments share a building or address with other non-software establishments. We refer to the industries to which these other establishments belong as "control industries." There about 700 industries on this list (at the 6-digit level) which constitute approximately half of all industries in the State of Texas (and most of the employment).[9] We take it as evidence that any location that contains an establishment in these other control industries is a location where a software establishment could potentially be situated (though we recognize in our estimation approach that the probability of doing so is not the same across all such locations). In other words, we assume that the physical infrastructure embodied in a building is fungible across these industries and zoning laws will accommodate software publishing activities.

Obviously, we do not mean to imply that if we observe that a software firm somewhere shares a building

[9]The reason for the large number of industries that share facilities with software firms is that many of industrial and agricultural firms also have office operations located separately from the production facilities. These offices are sometimes in the same building as software establishments. A weight scheme, which we describe at the end of this section, mitigates any associated issues when these industries are used to construct control variables.

with an advertising agency that every building that houses an advertising agency would potentially house a software firm. But most such buildings would be a potential location or most such buildings would at least be in close proximity to other buildings that would contain suitable space for a software firm. Moreover, it is extremely unlikely that a software firm would locate in an area that does not contain at least one establishment on this long list of control industries. This recognition then provides a reasonable starting point for identifying plausible locations that could potentially house software firms.

We then take all establishments in the control industries that have ever operated during our sample and retain their coordinates (dropping duplicates). We sorted these establishments by longitude (from west to east) and went sequentially through them dropping any establishment that was closer than 2 miles to a previously selected establishment. We obtain a final sample of 9,299 establishments that form the center points of circle locations of one mile radius. These locations cover about 11 percent of the area of Texas and are shown in Figure 2, Panel A (for the entire state of Texas) and Panel B for the Dallas-Fort Worth area.[10] Note that a location is in our sample even if it contained a control industry facility for only part of our sample. Therefore, when these locations are used in a panel, as in the entry analysis, they yield a balanced panel of locations. For example, a location that contained no control firms in the first two years of the sample will be in the sample during those first two years as a potential entry location, thus permitting a software publisher to be the first entrant in that location.

These locations are surrounded by concentric rings of 5, 10 and 25 mile radii. The areas within the inner 1 mile radius are non-over-lapping. The choice of ring radii reflects our interest in estimating the extent of localization externalities within these symmetric and equally-sized localities. The 25 mile radius approximates a county-level analysis.[11] It will also capture most of the areas of Texas MSA's. Our choice to further sub-divide at 5 and 10 mile radii reflects a "windshield" observation of the urban landscape in Texas cities. That is, commercial (high rise) clusters are distributed discretely across the urban landscape. The intermediate rings of 1-5 and 5-10 miles capture the concentration of similar firms in adjacent commercial clusters or isolated firms in the space between clusters. However, these mid-range divisions do not capture all firms within the city or urban region. Finally, the inner circle of 1 mile radius is intended to reflect "pockets" or neighborhood concentrations. A two mile diameter captures a majority of most central business districts.

[10] The final list of establishments depends on how the initial sort is made, as the maximal set of establishment locations with mutual distances that exceed 2 miles is not unique. For example, when the initial sort is on the basis of latitude (from south to north) the number of establishments we obtain differs somewhat. However, the covered area differs very little, as we pick up most of Texas that contains commercial activity.

[11] The majority of Texas counties are approximately square. The average shortest distance from the center point in the county to the county line is approximately 22 miles.

When used in a panel over our 28 quarter sample period, this results in 251,073 observations (27 quarters are used in the regressions, since the first quarter is dropped because of one quarter lagging of some variables). In Table 1 Panel B we report the summary statistics for these non-overlapping locations. We note that nearly all software establishments are less than a mile from a control industry firm, and thus these control industries effectively map out the large majority of potential locations.[12] However, after we drop locations to eliminate overlap, some gaps in space are created, and thus the proportion of software firms that are outside the final set of 1-mile circle locations rises to 28%. As will become apparent from the analysis that follows, this does not create any issues beyond the loss of these observations from the sample.[13]

We use two different concentration measures within each geographical area. One concentration measure is simply the number of firms in a given location. This is a count variable of the number of other (or rival) software establishments. The other measure of industry concentration is the number of employees in rival firms in a given location. There is of course no need to normalize by area size since each observation for each spatial division is in terms of identical areas as defined by the concentric rings.

The control industries are not only used in the definition of locations, but also as a control of the baseline propensity of software establishments to be situated (or enter) there. The necessity for such controls follows from the observation that, while those locations have some infrastructure that makes software establishment location possible, they differ by the extent to which they posses that infrastructure. Thus, the propensity of software firms to locate in these locations differs with the level of the locations' development. Locations that contain a large number of control industry employees or firms would, all things equal, be expected to contain more software establishments. Less developed locations, with a smaller number of control industry employees or firms, are, all else equal, likely to contain fewer software establishments.

Failure to control for this baseline might generate spurious clustering of entering firms since an area that is becoming more developed attracts more firms, including more software firms. For example, consider a tract of unimproved farmland that is developed and office parks are built. These office parks will be filled by a number of "white collar" employers, regardless of the presence or absence of spillovers. Thus, a positive spatial association at very small distances may be an artifact of land development patterns (or, equivalently,

[12] There are only 14 establishments for which the nearest control firm is more than 1 mile away. Most of those are one person operations (likely a free-lancer working from home).

[13] The only way to avoid gaps that potentially contain software firms is to divide Texas into a fine grid, creating squares of (say) one mile width, and dropping those that do not contain any control establishment. However, these squares would in general not have a control establishment at their center, and would cover a much larger portion of the state. Moreover, it would be geometrically challenging to define neighborhoods around square locations that contain all points that are no more than a specified distance from the edge of the location.

the abandonment of commercial land that has become less desirable economically).

3.2 Data and Variables

All econometric models utilize a common underlying primary dataset and share a number of independent variables. For that reason, we first provide an overview of the data and the construction of the common set of independent variables with a more tailored discussion to follow in the sub-sections that pertain to the growth, entry and exit analyses.

The establishment data used in this study are from the Quarterly Census of Employment and Wages (QCEW) compiled by the Texas Workforce Commission. This dataset provides establishment-specific monthly employment and quarterly total wages reported for all firms as required under the Texas Unemployment Insurance (UI) program. Each record includes the specific location (address and latitude/longitude) of the establishment, business start-up date (the date on which UI liability begins), and the relevant six-digit North American Industrial Classification System (NAICS) code. Separate establishments (branches or franchises) of the same firm are identified and reported in separate records. This highly detailed panel dataset is comprised of observations from the first quarter of 2000 through the fourth quarter of 2006 and allows us to map the locations and calculate distances between any two establishments.[14]

After restricting the analysis to the software publishing industry (NAICS code 511210), the sample has more than 15,900 observations, with 957 establishments corresponding to 877 unique firms (the vast majority of firms are single-establishment enterprises, and thus establishments will be used inter-changeably with firms). Average firm size is relatively small, around 35 employees.[15] The number of software publishing firms decreased from a high of 648 in Q4:2002 and Q1:2003 to a low of 581 in Q4:2006 (there were 526 firms in Q2:2000).[16] However, employment in the industry increased from 16,600 to 21,000 over these seven years, implying an increase in average establishment size. Key features of the data are provided in Table 1. Within one mile of an existing software firm, there are, on average, approximately 10 other software firms employing 394 employees. At distances between 1 and 5 miles, there are an additional 48 software firms employing 1,857 employees, a substantial drop off in the density of software firms (recall that area is proportional to the square of distance). The next 5 and the following 15 miles contain 46 and 72 software firms employing

[14]The authors obtained these data under an agreement of confidentiality and, therefore, disclosure of the actual data is subject to certain restrictions. While the data is restricted to the State of Texas, it should be borne in mind that Texas is a large economy. Over the period of analysis, the Texas economy was the second largest among US states, and would rank in 2008 as the 15th largest economy in the world. Moreover, Texas is geographically both extensive and diverse.

[15]Acs et al. (1994) find that smaller firms may benefit disproportionately from knowledge spillovers.

[16]There were 14 software publishing establishments with PO Box as the official address, and for which physical location could not be ascertained. These are not included in the above totals or in the subsequent analysis.

1,810 and 2,525 employees, respectively. These correspond to ever larger drop-offs in density.

There are 9,936,068 observations of control industry establishments, which correspond to 580,375 unique establishment locations (including establishments that entered or exited during our sample period).[17] Control industry employment goes from approximately 16,500 within a mile of a software establishment, to 155,000 in the next 4 miles, 314,000 in the next 5, and 303,000 in the next 15, reflecting the localized "strip mall" nature of commercial development in Texas and the average size of Texas cities.[18] At small scales (up to 10 miles), the employment of software firms appears to drop off faster than that of the control of firms.

Control for the baseline propensity of firms to be situated (or enter) in a given location is done in two ways. The first way is to use the number of employees in the other (control) industries as a control variable. The second way is to use the weighted number of employees as a control variable, with weights obtained from how frequently establishments of each industry are co-located with software establishments. In this second approach, the employees of each industry have an industry specific weight, which is the fraction of facilities in that industry that are co-located with software facilities. We find the second approach more appealing and report the results using the weighted controls in the paper. However, the results using the unweighted number of employees are qualitatively similar to those using the weighted measures (and are available from the authors).

Considering firm characteristics, we include a measure of the firm's exposure to university R&D funding, which captures the possibility that knowledge spillovers are available from research universities.[19] This variable is measured as total federally funded research expenditures at the university located closest to the firm, using the main address for the university campus. In order to introduce distance decay in the university R&D expenditures, we deflate total R&D expenditures by distance in miles (minimum 1 mile).

[17]There were 11,791 establishments with Post Office addresses. There is a similarly steep drop-off over these distances in the density of firms in the white collar service industries that form our control group. The location of almost all of those was obtained via batchgeo.com, and are thus utilized for the purpose of controlling for localized economic activity and infrastructure.

[18]One might be surprised by the large employment counts, which amount to nearly 800,000 employees within 25 miles of the average establishment. The reason is that these averages are taken at the establishment-cross-quarter level. With most software firms being located in the metropolitan areas (especially Dallas Metroplex and Austin), the typical urban density dominates the data. Note that in Dallas county there are approximately 1.3 million employees in the control industries (about 1.4 million total employment), the Houston numbers are similar, while those for Austin (Travis county) are 540,000 and 560,000, respectively. The bulk of employment in those areas is in the control industries. For the entire State of Texas, approximately nine-tenths of the employees are in the control industries, something that we discuss in section 3.1.

[19]Note that we assume that knowledge spillovers from universities are proportional to the level of research conducted at the institution. We proxy the level of research activity within the knowledge centers by using total federal research awards (by federal fiscal year) to Texas universities and research institutions for Science and Engineering R&D. Data on annual University R&D expenditures were obtained from the National Science Foundation (NSF). The annual NSF data actually span two calendar years, since the federal fiscal year begins in October. In order to convert these annual R&D expenditures into quarterly data, we use a fourth of a given year's total for each of Quarters 1-3, and a fourth of the following year's total for Quarter 4. We aggregate total federal awards by all granting agencies, i.e., DoE, EPA, DoD, by geographically distinct institution, i.e., system campuses are scored geographically separately.

We distinguish between research universities and colleges/junior colleges. Colleges may be smaller 4-year or 2-year degree-granting institutions. College funding is treated similarly to university funding. We include junior college funding since previous studies have found this variable to be more important than university research funding in explaining high-tech firm location decisions (Abramovsky, 2007).

We proxy the quality of the labor pool and the ability of firms to attract skilled employees by including a measure of local recreational and cultural amenities. As Woodward et al. (2006) suggest, cultural and natural amenities are important to attraction and retention of skilled workers. To measure the relative local presence of these amenities, we compute the share of county employment in NAICS 71, Arts, Entertainment, and Recreation, and NAICS 721110 (hotels and motels), 722110 (full service restaurants), and 722410 (drinking places, alcoholic beverages) as reported in the QCEW data set. A similar measure has been used by De Silva and McComb (2012).

To account for factor costs, we use the average quarterly payroll of high-tech industries in the county.[20] The county unemployment rate for the final month in each quarter, as reported by the Texas Workforce Commission, is included to provide an indication of the overall economic conditions in the local county. Summary statistics for these variables are given in Table 1. In the bulk of the regression analysis, we exclude firms that have only a single employee throughout the sample period.[21]

3.3 Some Facts about Software Firms and their Spatial Distribution

Before proceeding to formal econometric analysis, it is worthwhile to describe some key characteristics of software firms and their spatial agglomeration. Software firms are not completely undifferentiated as they tend to specialize in specific market segments. An important question is whether software firms in the same market segment tend to concentrate in particular locations. This is important because, if true, it would suggest that any human capital spillovers may be specific rather than general. It would also suggest that co-located entry might be driven to some extent by employees of a firm who launch a start-up in the same or a proximate building. Moreover, the interpretation of some of the findings might be affected.

In particular, we assume that software firms do not compete with each other in the local market, and therefore that local demand effects and supply considerations have no bearing on the spatial association of outcomes. This is a reasonable assumption. If, however, software firms that are in the same market

[20] We considered the inclusion of the undeveloped land price, but that measure was not available even at the county level, and probably a poor proxy for the software industry given the other controls.

[21] There are only eight such firms, but five of them are entrants. They are included in the entry analysis, since they may inform the decision of where to locate.

segment are co-located (for whatever reason), then national swings in the demand and supply of these products would induce a spatial correlation in outcomes. For example, if firms producing Voice Over Internet Protocol software were co-located, and the demand for VoIP software was to increase, resulting in both growth and entry of firms in that market segment, then our entry analysis would infer a spatial association between employment in a location (and implicitly, employment growth) and entry.[22]

Table 2 provides information on the location of individual firms that can help assess this localization, albeit informally. We randomly select five firms in separate cities, labeled A, B, C, D, and E. Then, for each of these five firms, we list all the other software publishing establishments located less than five miles away. We indicate whether firms share the same building, their primary business function, their average employment level, and the years they were operating.[23] The firm and city information needs to be anonymized to conform to the requirements of obtaining the QCEW data, but the information in the table is still instructive.

There are seven pairs of firms that located in the same address. Looking first at these pairs of co-located firms, we see that in two cases a building contains a large firm along with a much smaller one. But the smaller firm does not appear to be an off-shoot of the larger firm; in both of these pairs, the firms are in different software categories. Overall, taking all seven pairs of co-located firms, there is one pair of similarly sized firms that belong in a similar market segment, and one pair that seems to have generic labels (and where similarity cannot be fully assessed). Most of the other pairs are clearly discordant in terms of market segment. To expand our sample of co-located firms, we also looked for such firms in selected locations other than the five cities reported in Table 2. This more than doubled the sample of co-located firms, but resulted in the same qualitative pattern.[24] There are now proportionately slightly more pairs of large and small co-located firms than in Table 2, but all pairs except one consist of firms in different business segments (these firm pairs are not reported in the interest of conserving space, but are available from the authors).

Though the sample is small, and we cannot make any formal inferences, it appears that there might possibly be some minimal agglomeration of firms in the same line of business at the building level. But such

[22] If indeed observed, such a co-location of firms in the same market segment might itself indicate some spatial interaction that is market-segment specific. More importantly for our econometric analysis, if establishments in some business segments tend to be spatially concentrated while establishments in other segments tend not to be spatially concentrated, then the increase in the relative market size of the former business segments would result in a correlation between growth, entry, and concentration. However, this "double hypothetical" is very unlikely.

[23] The primary business function was obtained by searching for each firm on the Internet. This labor-intensive procedure cannot be replicated for every single facility in our dataset. For this reason, this evidence is presented for a small sample and the analysis is somewhat informal by necessity. For city B, there were approximately 25 firms within 5 miles. Thus, we report firms within 1 mile of selected location.

[24] In total, we examined approximately 40 of the 181 co-located firms in the entire state of Texas.

agglomeration, if indeed present, is very small. Moreover, there seems to be no evidence of co-located firms being spin-offs of existing firms entering the same business segments as the originating firm, though some co-located firms may be spin-offs venturing into different business segments.[25]

Let us now turn to the examination of the firms in each of the five mile radius circles reported in Table 2. An examination of the descriptions of the business segments to which the firms belong to reveals some categories that are more common than others (e.g., “computer software” or the “business” descriptor) perhaps because of the more generic nature of these labels. In city A, the firm in the “bull’s eye” is one such generic producer of both software and services. Two other firms in its area list computer software in their business category, but both omit the provision of services. In city B, the selected firm is in Electronic Trading Software, an uncommon category. A nearby firm seems to be in a related market segment, but the others are very distinct. The firm in city C is a service provider; 3 of the other 15 software firms in each area emphasize the provision of services, but so do some firms in the other listed cities. In city D, a firm focusing on “Business solutions” is located close to another two firms in related market segments. Finally, in city E, a firm specializing in Entertainment software is located close to another firm in the same business; but that firm was tiny and lasted only three months (clearly a failed attempt to launch a business). We repeated the exercise for a random firm located in the Dallas metropolitan area, where the number of firms within the five-mile radius is too large to report in this table (we can make this separately available upon request). The similarity of firms within five miles of the selected firm was very small, even though that firm specializes in computer game software, which is not a “rare” category. Only one of 31 firms located within five miles was a computer game developer. Moreover, the list of firms does not seem dominated by any particular software category. Though there are a number of categories with more than one firm, this would be expected simply by chance.

To summarize, while the totality of the evidence is against the notion that there is strong spatial specialization, a small amount of such specialization cannot be ruled out. It appears that a firm is slightly more likely to be surrounded by firms in the same business segment as itself, but the large majority of software firms in the same area are engaged in a different part of the software publishing business. It is thus unlikely that co-location is driven by spin-offs or by human capital that is specific to a market-segment.

[25] Buenstorf and Klepper (2009) have demonstrated in their study of the Akron tyre cluster that organization replication can generate a spatial pattern akin to that arising from agglomeration economies. This seems to not be the case in the software industry.

4 Econometric Analysis and Results

4.1 Spatial Persistence in Software Industry Employment

The software industry is very dynamic. There is a large turn-over rate of establishments and substantial changes in the scale of establishments over time. For example, approximately half of the establishments that were in operation at the start of 2000 had exited by the end of our sample in 2006, while many of the establishments that did not exit experienced large drops in employment. All in all, from the 16,645 jobs that were initially in our dataset, only 7,015 (or about 41 percent) persisted in the same establishment until the end of the period.[26] The persistence of jobs is not the same as worker turnover; a worker who leaves an establishment and gets replaced by another worker at that same establishment during our sample period registers as a retention of that job by that establishment.

During the same time frame, 372 new establishments entered (almost all in different buildings than the exiting establishments). The jobs created by these entrants and the jobs added by growing incumbent establishments raised total employment in the industry to about 21,000 (an increase of 24%). This means that only one third of the jobs at the end of the period were jobs that existed in the same establishment at the start of the period. Given that this industry does not rely on specialized infrastructure and has non-localized demand for its product, and given the entrants typically choose different addresses than incumbent or exiting firms, there is a potential for the spatial distribution of this industry to be completely transformed.

This turns out not to be the case. At the macro level, a quick way to assess whether spatial concentration has increased is to investigate whether the share of employment in, say, the top five counties has increased over this period. The identity of the top five counties has remained the same (Dallas, Travis, Harris, Collin, and Bexar). The number of software publishing employees in those five counties has increased at approximately the same rate as in entire state of Texas, marginally raising their combined share from about 89% at the start of the period to about 90% at the end. It is worthwhile to point out that, while these figures indicate that concentration is broadly constant at the county level, they do not provide any direct evidence about concentration at the 1-mile radius level.

Some evidence at the micro level can be provided by a pictorial examination of a few representative areas. One cannot easily plot employment into space, but can plot establishments. In Figures 3 and 4, we have plotted the software publishers and the control firms in Austin and North Dallas areas for the first

[26] This percentage is equal to $(\sum_i min\{emp_{i,1}, emp_{i,T}\})/(\sum_i emp_{i,1})$ where $emp_{i,1}$ is an establishment's employment in the first quarter of our sample (and 0 if the establishment entered at a later date) and $emp_{i,T}$ is an establishment's employment at the last quarter of our sample (and 0 if it exited by that quarter). Note that by construction this ratio cannot exceed unity.

and last quarter of our sample. The distribution of control firms indicates the areas near which software publishers could be located. The first thing to note is that software publishers are not uniformly distributed in this space, but are rather concentrated in certain areas. The second thing to note is that the areas of concentration remain stable despite entry and exit: an area with prior concentration of software publishers retains that concentration to the end of our sample period. A final observation relates to newly developed locations. It is clear that some areas that were not commercially developed in the second quarter of 2000 became developed by the end of 2006. However, these areas do not seem to have also spawned a cluster of software firms. Moreover, isolated software firm entry into these newly developed areas seems rare.

Some additional evidence at the micro level is obtained by computing the number of software employees in each of the 1-mile radius locations we have defined and then see how that number changed over our sample period. The top ten 1-mile radius locations in terms of initial employment contained 61% of software employees in our initial quarter. The corresponding percentage at the end of our sample period is 65%, an increase in concentration despite an increase in the number of establishments and the number of employees. However, there has been some churning of the top 10 locations. The percentage of software employees employed in the end of the sample period in the top 10 locations measured by employment at the start of the sample period is 48%, a decline from the initial level, but still a remarkable persistence in such a dynamic industry. Removing from all calculations one outlier location which jumped to first place from outside the top 10 list due to the entry of a large facility, the percentage increases to 57%, essentially unchanged from the initial level of 61% despite the dramatic changes in the landscape of the industry!

A more systematic and formal analysis would consider all locations with positive employment at the start of the sample period and account for other changes in these locations that would be expected to affect end-of-period employment. The conceptual exercise we want to perform is the following. Suppose we could exogenously increase the software employment in a particular location at the start of the period by one percent (by, for example, increasing the order flow of the establishments situated there). What would then be the percentage increase in employment in that location at the end of the sample period?[27]

Of course, we do not observe software jobs being exogenously created in a location and measure their impact on that location's jobs at the end of the sample period. Rather, we observe locations that differ in the level of initial software employment and other characteristics. Our attempt, using locational controls

[27] The same hypothetical question could be expressed in terms of employment levels rather than percentages, in which case it would correspond to analysis using the number of employees rather than their log. We discuss this alternative analysis below.

based on industries that are typically co-located, along with the fact that the software industry has minimal infrastructure demands and a non-physical product with a national market, ensures that the initial variation in employment comes close to being exogenous in the statistical sense.[28]

To better understand the content of the question we investigate, we observe that end-of-period employment in a location is a function of jobs lost and accrued. In all of this analysis, a job in an establishment is retained regardless of employee turnover. For the moment, suppose that every job (or position) in all establishments has the same probability of being lost and that job accrual is proportional to the initial number of jobs in a location. Then the elasticity of final period employment with respect to initial employment is 1. However, such an extreme degree of persistence is unlikely. Entrants, for example, are more likely to locate in places with more existing software jobs (conditional on overall economic activity), but not proportionately more likely. Similarly, establishment job growth is not proportional to establishment size (as shown by the voluminous literature on Gibrat's law) and hence final employment in a location is not expected to be proportional to initial employment in that location. As a result, there would be some shift in the landscape of software industry activity over time. Some areas with high concentrations would "revert to the mean" or closer to a level of software industry activity that is in proportion to overall economic activity in that location. Other areas with limited activity might exhibit higher concentration for idiosyncratic reasons.

Spatial persistence would be higher than this benchmark if establishments located in areas with high concentrations of software firms grew systematically faster than establishments located in areas with low concentrations. The degree of spatial persistence of employment would balance these factors. In the absence of agglomeration economies, we would expect spatial persistence be driven solely from the inertia of jobs at the establishment level.[29] With agglomeration economies, we would expect it to be higher than this value. It is worth pointing out that the observed fraction of initial jobs that remain with the same employer is so low as to be consistent with even a *negative* elasticity.[30] Thus, even a demonstration that the elasticity of final employment to initial employment is positive has empirical significance.

We now describe the econometric framework through which we investigate the extent of spatial persistence. Our spatial unit of econometric analysis is the one mile radius locations described in section 3.1. For

[28] It is not required that initial variation in employment be random, but only that it is uncorrelated with unobserved shocks to end-of-sample period employment.

[29] This assumes that locations are "small" as a fraction of the industry employment.

[30] Consider two establishments (or locations), one with initial employment of 100, the other with initial employment of 200. Let the final period employment levels be flipped. Then, the number of retained jobs is 100 for both the initially small and the initial large establishment, or a total of 200 (67 percent) for the industry. But the elasticity of final employment with respect to initial employment is negative.!

each of these locations, we estimate the end-of-period employment in the software industry as a function of initial employment and other initial conditions. We use as initial conditions the number of software firms in the location at the start of the sample period, the number of software firms in concentric 1-5, 5-10 and 10-25 mile rings and the number of employees of these firms. Though the 1-mile radius locations are not overlapping, the surrounding rings are, as illustrated in Figure 5. Panel A shows the locations in the Dallas area and Panel B shows the surrounding rings in two of those locations. The overlap of the rings does not raise any econometric issues.

County effects for the five counties with major employment in this industry are included in some specifications.[31] For these regressions, the effect of initial conditions is identified from the within-county distribution of software publishing firms. For reasons explained above, we include the (weighted) number of employees in the set of control industries in the initial and final periods as explanatory variables in a number of specifications. In addition to these exogenous variables, we sometimes also include the final period number of firms and employees in the concentric rings surrounding a location. These latter variables are co-determined to some extent (though the stronger influence probably goes from the larger outer rings to the center). More formally, our most general regression model is given by

$$Y_{l,T} = D'_{l,t=1}\delta_0 + R'_{l,t=1}\rho_0 + C'_{c_l,t=1}\varphi_0 + D'_{l,T-1}\delta_1 + R'_{l,T-1}\rho_1 + C'_{c_l,T-1}\varphi_1 + \eta_l \quad (1)$$

where $Y_{l,T}$ is the location l's quarterly software employment in the final quarter, T (Q4:2006), or its natural log. The vectors $D_{l,t=1}$ and $D_{l,T-1}$ represent initial and final period (minus a lag) density variables for location l, respectively. Similarly, we include initial and final period (minus a lag) location variables ($R_{l,t=1}$ and $R_{l,T-1}$), and variables specific to the county c_l where this location is situated ($C_{c_l,t=1}$ and $C_{c_l,T-1}$). Only locations with positive initial software industry employment are included in this regression. We base all inferences on robust standard errors for parameters and marginal effects.

Employment has a long right tail, and many of the explanatory factors are expected to act synergistically, rather than in a purely additive fashion. Thus, an analysis using employment (rather than its log) as the dependent variable is less appropriate and results in larger standard errors, though the main conclusion with regards to employment persistence is robust (we report results using both). The lowest possible employment level is 1, so $Y_{l,T}$ is censored and equation (1) is estimated using censored regression methods.[32] Note that

[31]The top 5 counties for which we include dummy variables are Dallas, Travis, Harris, Collin and Bexar. Using country fixed effects for all counties effectively "dummies out" most locations situated in those counties. For sensitivity analysis, we also estimated those models, resulting in no major changes in the findings.

[32]The censoring threshold in the log linear specification is 0, while in the linear specification it is 1. We distinguish between

one quarter of the observations are censored, which might seem a bit high given the large persistence in employment. However, about 70 percent of the locations have only one firm and these locations form the bulk of those that are censored (i.e., censoring essentially implies that the sole establishment in that area has exited and has not been replaced). When viewed in this way, and given that one-half of the incumbent establishments fail, the censoring fraction appears low. This implies that, in many locations with only one establishment, at least one other establishment was attracted prior to the exit of the incumbent establishment. This is remarkable given that the locations with software firm activity are but a very small fraction of possible locations that software firms can choose to locate, revealing the propensity of entrants to locate in close proximity to incumbents (as will be formally demonstrated in section 4.2).

Table 3 presents the censored regression results using log employment as the dependent variable, generally moving from the simplest to the more complicated specifications. Since our focus is on the marginal effect of initial employment on the expected value of final employment, this table (and all tables with employment as the dependent variable) report these marginal effects and the associated robust standard errors.[33] Table 4 presents the results using the same sets of regressors but with linear rather than log specifications. In order to make a valid comparison between the marginal effects in the two tables, those of the regressions in Table 4 are weighted by the initial number of employees in each location, i.e., they are the sample average of $\frac{Y_{l,t=0}}{Y_{t=0}} \frac{\partial E[Y_{l,T}]}{\partial x_l}$ where $Y_{t=0}$ is the initial software employment in Texas and x_l is any regressor.

We discuss the results of Tables 3 and 4 together. In the simplest specification (model 1), no covariates are used except for differential intercepts for major counties. The point estimates suggest than a one percent increase in initial employment translates into 0.66-0.75 percent increase in final period employment. This indicates a very large spatial persistence in line with the informal evidence described earlier. The underlying parameter estimates are higher still, and not significantly different from 1! The addition of software employment in close and moderate proximity and other location characteristics (model 2) has no material impact on the elasticity estimate. Interestingly, while location characteristics are jointly significant, employment at any distance up to 25 miles is not. Controlling for localized employment and growth in the control industries (model 3) also leads to only minor reductions in the elasticity estimate. Adding current location conditions to the regression (model 4) shows that final employment in a location increases with initial proximate employment but decreases in final proximate employment (lagged by one quarter). Because

locations that report one employee and censored locations were there is no software employment (less than 1 employee).

[33]The reported values in the table are the sample average of $\frac{\partial E[\ln(Y_{l,T})]}{\partial x_l}$ where x_l is any regressor. The tables with the underlying coefficients are available from the authors.

employment in a mile-radius location is typically a small fraction of the employment in the surrounding 25 radius ring, we posit any causal effects go mostly from the surrounding area to the inner circle. Thus, locations in moderate proximity to an existing software cluster grow faster, but those in close proximity to a growing cluster grow slower. Knowledge and other productivity enhancing spillovers are unlikely to yield this pattern (software activity, whether initial or final period, should increase employment in a location), but can easily be rationalized with a labor pool thesis. Establishments in a location proximate to areas with many software employees can obtain a ready supply of workers, but if those proximate areas are growing themselves, they can siphon workers away from that location.

The last two models (5 and 6) augment these specifications by adding the number of establishments at various distance thresholds as explanatory variables. In these regressions, agglomeration effects are decomposed to those arising from an increase in the number of facilities (holding employment constant) and those arising from an increase in employment (holding the number of facilities constant). Both appear generally significant. In the log specification (where the coefficients are both elasticities) the estimated coefficient on the number of firms is larger (though less precisely estimated). Remarkably, looking at the results of Table 4, a replication of the facilities in a location, i.e., doubling the number of facilities active at the start of the period, seems to double end of period employment in that location). Part of the explanation why the number of initially active establishments is associated with higher terminal employment is that it increases the probability that some of these establishments become successful and grow substantially and reduces the possibility that all of them fail.[34] Estimates at higher distances are zero, except those for the 5 to 10 mile range (but where the number of employees and number of establishments enter with opposite sign in the log specification). When including final employment and number of firms (model 6), results are the same as those for the conceptually similar model 4 discussed earlier.

In light of the discussion at the start of this section, the magnitude of the spatial persistence of employment appears "large." But to better assess how much larger it is compared to a benchmark of no locational advantage from any initial industry presence, we compare the results in Tables 3 and 4 with the counterpart specifications estimated at the establishment, rather than the location level. The sole modification is that, in some instances, we include both the location and facility initial employment levels.[35] These specifications,

[34] We have verified this in our data, but do not report the results for brevity.

[35] It is important to note that the intercept of the location employment and establishment employment regressions are not comparable. If the elasticity with respect to initial employment were fixed to 1, the intercepts in the former models reflect the growth rate of software employment in a typical location, while in the latter models they reflect the growth rate of a typical establishment.

reported in Tables 5 and 6, have some similarity to employment growth regressions, except they are estimated as a cross-section regression on the basis of a single employment change, rather than as a dynamic panel. An important advantage of our approach is that the elasticity estimates are directly comparable with the locational persistence of employment specifications. Another advantage of this approach is that it side-steps many of the econometric issues of estimating dynamic panel models (especially when fixed effects are included) and eliminates the need to consider entry (which is investigated separately below).

Examining model 1 of Tables 5 and 6, which is the direct counterpart of model 1 of Tables 3 and 4, the estimate of the coefficient of initial establishment employment on final period establishment employment is much smaller than those obtained at the location level.[36] Increasing initial employment in an establishment by one percent leads to only 0.3 to 0.4 percent increase in final employment. Models 2, 3 and 4 are progressively more inclusive specifications, and confirm this pattern. The results of these models corroborate the conclusion that establishment level employment persistence is much lower than location level persistence.[37]

Conceptually, the difference between firm and location employment persistence could consist of three components: (i) faster growth rates of firms in locations with higher firm concentration, (ii) the capture of jobs lost to an establishment by other establishments in that same location, and (iii) higher entry rates of firms in locations with prior software presence and hence the creation of more jobs by entrants in high initial employment locations. The first component is evidence of positive spillovers from co-located firms, which would yield higher end-of-period employment among firms present in a location with many other co-located firms. If that were the case, then exogenously increasing the employment level of a single establishment would lead to a smaller increase in its final employment than exogenously increasing the employment levels of all establishments in a location.

We can investigate this key question using establishment-level regressions by re-estimated models 1-4 of Tables 5 and 6 after adding the initial employment by co-located establishments as a regressor (plus 1, when taking the log). The results, reported in columns 5-8 of Tables 5 and 6, suggest that there are no such positive synergistic effects. In fact, if anything, there seems to be a negative effect from the presence of

[36] In fact, much smaller than unity. A coefficient of 1 would imply Gibrat's law. Most of the literature investigating the premise in Gibrat's law that growth rates are independent of firm size has either found a negative association, e.g., a mean reversion effect where large firms grow slowly while small firms grow faster (see early work by Evans, 1987, and Dunne, Roberts, and Samuelson 1989, as well as later work by Hart and Oulton, 1996, and Dunne and Hughes, 1994).

[37] The number of other software establishments in the same location seems to have no effect on final employment of an establishment, when conditioning on that establishment's initial employment. The relationship is positive and robustly significant when we do not condition on an establishment's initial employment (regression not reported). This confirms the findings of Holmes and Stevens (2002) who find that average establishment size is larger in localities with more establishments. In other words, an area with an industrial agglomeration measured by number of establishments in that industry is even more agglomerated when measured on the basis of employees.

other firms (measured by their employees) on an establishment's final period employment. With the addition of both number and employment of other establishments in the specification, the sign on the number of other establishments is positive (though significant only in logs). However, the coefficient on the number of employees of those establishments becomes even more negative.[38] This means, then, that the effect of initial employment on final employment does not arise from a positive effect that initial employment has on the growth of the existing establishments. It rather arises because more of the jobs that existing establishments lose are captured by co-located establishments and because more jobs are created by entrants.

We next examine whether local capture of jobs lost by an establishment is indeed a contributor to local employment persistence. This is done by re-estimating the regressions in Tables 3 and 4 using as the dependent variable the final employment in a location in establishments that were present at the start of the sample period (incumbent establishments). As reported in Tables 7 and 8, the estimated coefficients on initial employment are higher than those of Tables 5 and 6. This indicates that the negative effect of co-location on individual establishment growth is outweighed by the tendency of jobs to remain in areas with larger prior employment.[39]

Perhaps more importantly, the coefficient on initial employment when the dependent variable is the end period incumbent establishment employment is lower than the elasticities reported in Tables 3 and 4. For example, Model 1 of Table 3 yields a elasticity of 0.656, while the elasticity is 0.528 when incumbent establishment employment is the dependent variable (Table 7) , and 0.366 at the establishment level (Table 5). For the linear version of this model, the elasticity measured by the weighted marginal effect is 0.753 when employment of all establishments is the dependent variable (Table 4) versus 0.539 when incumbent establishment employment is the dependent variable (Table 8). This compares to 0.283 at the establishment level (Table 6). Similar results are obtained when comparing the other models of these tables. In other words, the employment persistence in a locality is only partially driven by incumbent firms capturing jobs lost to other incumbent firms. Some of the persistence must be driven by a disproportionate entry of firms into locations with greater initial presence of software publishers.

[38] When adding the employment of collocated software establishments, the association between initial and final establishment employment strengthens somewhat.

[39] One might at first find this paradoxical, but it not. For example, consider four establishments, A, B, C1 and C2, operating in locations A, B, and C. Suppose establishments A and C1 have initial employment of 100 while establishments B and C2 have initial employment of 50. Let there be a simple process of mean reversion, whereby the large establishments lose 10 workers who get hired by the small establishments. Let establishment C1 lose another 5 workers, so that establishment employment growth is negatively associated with location employment. Then, the elasticity of final employment with respect to initial employment is around 0.54 at the establishment level, and around 0.78 at the location level, because many jobs lost by the large establishment in the high employment location are balanced by gains in the small establishment in that same location.

In the next section, we investigate and measure the extent to which software industry activity in a location makes that location more likely to attract new software establishments.

4.2 Entry

In this section, we take a detailed look into the entry process of establishments in order to confirm its impact on the spatial persistence of industry employment. Establishment entry is defined as the introduction date of a new Enterprise Identification Number (EIN) in our dataset. Our primary aim is to understand the extent to which the prior presence of software publishing firms in a location influences the entry rate of other software firms in that same location. Since other factors are associated with entry in a particular location, care needs to be exercised in formulating an appropriate econometric design. As elsewhere in the paper, our analysis is reduced form, i.e., we estimate the number of entrants in a particular location as a function of location characteristics.

Even under the null hypothesis that localization economies do not influence entry probabilities, the expected number of entrants is not uniform across all geographies defined by land area. Ideally, our unit of analysis would consist of small areas that contain suitable locations. In Section 3.1 we described the identification of a set of non-overlapping locations (one mile in radius) that are potentially suitable for software establishment entry. We estimate the expected number of software firms entering into any of these locations as a function of location characteristics similar to those used in the analysis of employment persistence. These characteristics include the number of software firms already present in that location, the number of people employed by these firms, the number of software firms (and employees) in a surrounding ring of 5 mile radius (not including the inner circle), the number of software firms and employees in a surrounding 10 mile radius ring (not including the inner 5 mile radius circle), the number of software firms and employees in a surrounding 25 mile radius ring (not including the inner 10 mile circle), the weighted number of employees by other related industry in that location (as used in the regression in section 4.1), and a number of county-level controls described in section 3.2. Not all of these characteristics are used in every regression. Of these characteristics, the number of employees in other related industries, weighted by each industry's spatial association with software publishing firms, is the most important control variable. As noted earlier, this is not considered a causal relationship, but functions as a summary statistic for the baseline propensity of software firms to enter in that location. The number of software firms (and employees) in surrounding locations measures the presence of less localized spillovers.

An important difference between the employment persistence analysis and the entry analysis is that in the latter we take advantage of the time variation in our data. We estimate the number of establishments entering in a location in a particular quarter as a function of the location characteristics in the preceding quarter. Doing so does not create any econometric issues and increases the variation we can exploit in our sample. In the typical entry case, there is a single software entrant in a given location in a given quarter, while the maximum number of entering software establishments is two (except for one occurrence of 5). In particular, recalling Table 1, there are about 0.001 software entrants per location each period in the full sample. Conditional upon there having been at least one entrant in the one mile radius, the average is 1.04.

Given these small counts, we have estimated the entry models using an ordered probit. Our dependent variable is the number of software publishing start-ups $y_{l,t}$ in a given one mile radius location l during a given quarter t. The basic ordered probit model is

$$Y^*_{l,t} = D'_{l,t-1}\delta + R'_{l,t-1}\rho + C'_{c_l,t-1}\varphi + \tau(t) + \alpha_{c_l} + \epsilon_{lt} \quad (2)$$

where $Y^*_{l,t}$ is a continuous latent variable with two threshold points, one delineating no entry from entry by a single establishment and the other delineating entry by one establishment from entry by two or more establishments. The independent variables can be classified into three main groups: $D_{l,t-1}$ represents software industry activity/density measures in the location, $R_{l,t-1}$ controls for other location specific variation, $C_{c_l,t-1}$ controls for county specific characteristics of the county c_l where location l is situated, and $\tau(t)$ is a quadratic function of time (the model cannot be meaningfully identified with time fixed effects). Most of the variables that may be characterized by long tails are in logs. When taking the log of the number of firms or the number of employees, the value of 1 is added to ensure well defined regressor values when no firms are present in a location. In some specifications we have included county effects α_{c_l} for the top 5 counties with the most software publishers.[40] The random disturbance ϵ_{lt} has a standard normal distribution.

The results are reported in Table 9. Localized own-industry density appears to have a strong positive effect on entry probabilities regardless of whether the number of software firms or the number of employees in those firms is used as a measure of density. The effect appears to be stronger for distances of less than one mile, somewhat important for intermediate distances (1-5 miles) and marginal or absent for distances greater than 5 miles (especially for the more comprehensive specifications). The use of county

[40] The remaining counties are pooled together as the excluded category. We use county effects rather than location effects to control for unobserved location heterogeneity, because multiple entries over the sample period into any particular location are rare. Moreover, the model cannot be identified with county effects for each county, without the loss of many observations.

fixed effects tends to weaken the association between pre-existing software firms and subsequent entry, but only marginally, while complementing these fixed effects with time varying location characteristics tends to have no effect. Reassuringly, our control for localized activity by firms that use similar infrastructure (Other Industry Employees) is positive and strongly significant. The number of other software firms seems equally important for the location decision of potential entrants as the total number of employees of those firms (measured by statistical significance). Moreover, the two sets of models have an approximately equally good fit as measured by the log likelihood, with the specification that uses the number of firms as the measure of software firm presence having a slight edge.

Finally, when both the number of firms and the number of their employees are used in the regression, significance drops substantially, especially for the number of employees present. For this reason, we attach greater importance to the results in the specifications (1) through (6) where either the number of establishments or the number of employees of these establishments is used as a control. From the remaining variables, high tech wages and, to a lesser extent, university spillovers are associated with higher entry probabilities (the former perhaps as a proxy for demand for high tech labor) while junior college spillovers and local unemployment rates are negatively associated with entry.[41]

Despite the very small entry counts, we also estimate the entry process using the Poisson model in order to investigate the sensitivity of the results to the econometric specification. This model is estimated via the pseudo Maximum Likelihood method to increase the robustness of the inference.[42] The estimated coefficients are identical to Poisson ML estimation but the standard errors are adjusted for over-dispersion and clustered at the county level. Our dependent variable is the same as in the ordered probit analysis, and its conditional mean is given by

$$E[y_{lt}|\alpha_l, D_{l,t-1}, R_{l,t-1}, C_{l,t-1}] = \exp(D'_{l,t-1}\delta + R'_{l,t-1}\rho + C'_{c_l,t-1}\varphi + \tau(t) + \alpha_{c_l}) \tag{3}$$

Estimation results for these generalized Poisson regressions are contained in Table 10. The parameter estimates of the Poisson and ordered probit models are not directly comparable with each other because of differences in scaling, but statistical significance and relative magnitudes can be compared. On this basis, the Poisson and ordered probit estimates for the agglomeration variables and their effect on entry

[41] We have also re-estimated these models using ordered logit. The ordered logit parameter estimates for the agglomeration variables and their effect on entry probabilities are qualitatively similar to those of the ordered probit results. The only noticeable difference is in the rate at which spillover effects decay with distance, which tends to be smaller under the ordered logit than the ordered probit. The two sets of results are also similar with regards to the auxiliary controls, except that university spillovers are positive significant under the ordered logit.

[42] For a more detailed discussion of this reasoning, see Wooldridge (2002) and Cameron and Trivedi (2005). Implementation is via the PPML command in STATA with the keep option.

probabilities are mostly similar. The only difference seems to be that localized own-industry density effects now appear slightly stronger for the 1 to 5 mile distance, as for distances of up to one mile, but only when employment levels are used as the measure of industry activity.[43]

To summarize, the presence of software establishments in an 1-mile radius location exerts a strong positive influence on the number of new software establishments entering that location. Software establishment presence in the surrounding 5 mile ring also exerts a substantial positive influence. There is a far smaller and inconsistent effect for distances further than 5 miles. These effects are robust to the addition of a number of other controls.

4.3 Firm Survival

In the preceding section, we have shown that locations with prior software establishment activity are more likely to attract new entrants. What has not yet been demonstrated is whether these entrants last at least as long, or longer, than the (fewer) entrants that choose to locate in other areas. If that were not the case, perhaps because areas with other software establishments have more fierce labor market competition, then these increased entry rates would not necessarily contribute to a greater number of active establishments in the longer term. In this section, we estimate software establishment survival rates, allowing the hazard rate for each establishment i to vary over time as a function of the establishment's age and also be a function of time-invariant covariates. For this analysis, observations consist of all of establishments i that entered after March 2000, eliminating any concerns about left censoring, and possible selection biases that might arise from it. Right censoring is accounted for in the estimation procedure, as explained below.

Two variables are particularly important: the survival time of establishment i, χ_i (which is equal to the establishment's age at exit or the number of quarters it is observed until the end of our sample), and the exit indicator d_i. Note that d_i takes the value 1 when the exit date of an establishment is observed, and 0 if the observation is right censored. The set of time invariant covariates is divided into three groups, as in the previous analyses, but pertains to the conditions present when the establishment enters the market, t_i. The first group, D_{l_i,t_i}, includes our usual density variables. The second group, F_{i,t_i}, includes firm specific variables such as (initial) size, and the final group C_{c_i,t_i} includes characteristics of county c_i where establishment i is located, such as the unemployment rate. In the expressions that follow, Z_{i,t_i} is used to

[43] A careful comparison between Tables 9 and 10 reveals that the specifications in the two tables are similar by not identical, thus increasing the range of models we have considered. In particular, the set of models with county fixed effects is not the same across the two tables. We have estimated using ordered probit the exact same set of specifications in Table 10, but no additional qualitative differences in the results emerge other than those already mentioned here (these results are not reported because they are largely redundant).

denote all three groups and the corresponding coefficient vector is represented by ψ. In our base analysis, we adopt the Weibull specification, where the hazard rate is

$$h(\chi_i, Z_{i,t_i}) = pe^{Z_{i,t_i}\psi}\left(e^{Z_{i,t_i}\psi}\chi_i\right)^{p-1} \tag{4}$$

while the survivor function (the probability of surviving χ_i periods) is

$$S(\chi_i, Z_{i,t_i}) = e^{\left(-e^{Z_{i,t_i}\psi}\chi_i\right)^p}. \tag{5}$$

The corresponding density function of survival times is

$$\begin{aligned} f(\chi_i, Z_{i,t_i}) &= S(\chi_i, Z_{i,t_i})h(\chi_i, Z_{i,t_i}) \\ &= e^{\left(-e^{Z_{i,t_i}\psi}\chi_i\right)^p} pe^{Z_{i,t_i}\psi}\left(e^{Z_{i,t_i}\psi}\chi_i\right)^{p-1}. \end{aligned} \tag{6}$$

where $\lambda_i = e^{Z_{i,t_i}\psi}$ is the location parameter and p is the shape parameter. Having defined $h(\chi_i, Z_{i,t_i})$, $f(\chi_i, Z_{i,t_i})$, and $S(\chi_i, Z_{i,t_i})$, we now write the likelihood for the Weibull model as follows:

$$L = \prod_{i=1}^{n} \left\{f(\chi_i, Z_{i,t_i})\right\}^{d_i} \left\{S(\chi_i, Z_{i,t_i})\right\}^{1-d_i}. \tag{7}$$

where n is the number of establishments used in the duration analysis.

The estimation results are reported in Table 11. There is no association between the number of other software firms or their employment and the exit rates of an establishment except for a very tenuous one that points to an increased hazard when an establishment enters in a location with a large number of software industry employees. But confidence in that finding is further undermined by the negative coefficient on the number of co-located establishments, which essentially cancels that effect out. In other words, the presence of other software publishers in a location does not seem to confer an advantage in terms of ability to survive and might actually confer a small disadvantage. Proximate alternative employers might hasten the demise of weak firms by enabling an easy transition of their employees to other firms.[44]

With regard to the other covariates, the size of the localized employment of the control industries is not expected to have any major impact on the profitability and survival of an existing establishment. Indeed, no effect is apparent for software firm exit rates. The hazard rate is increasing with establishment age ($p > 1$) but somewhat decreasing in initial establishment size.[45] However, it is important to note that firm

[44]Proximity results in small switching costs for the employee, and personal interactions between the employees of the two firms facilitates recruiting. An employee who is considering leaving a firm has detailed information about a proximate employer, while a proximate employer has good information about the prospective employee, increasing the chance of a good match.

[45]For example, Dunne, Roberts and Samuelson (1988, 1989) find the failure rates of both plants and firms decreases with the size and age of the enterprise. See also related work by Dunne, Klimek and Roberts (2005) on the role of firm experience.

size refers to initial size of the establishment, and an increased scale of operations may simply proxy for deeper financial resources. All other variables tend not to be significant, including the firm's initial wage. Wage rates are clearly endogenous. Higher wage rates might reflect the higher profitability of firms (more profitable firms are known to pay higher wages), or the higher quality of labor that these firms employ (which would seem to suggest that establishments with high quality labor are more likely to survive), or the need to offer higher wages to attract labor. Regardless, they seem to have no effect.

The only exception to this general non-significance is the county high-tech wage (which tends to increase exit rates) and the unemployment rate that also does so when used in conjunction with county wages. High industry wages may reflect competition for labor from other high tech industries and a high unemployment rate may reflect economic stress. To investigate the robustness of our key conclusions to the inclusion of the wage and employment levels, we have performed a series of robustness tests which are reported in Table 12. In particular, we have re-estimated many regressions with the county wage but without the own wage, and have also estimated regressions without either of the two wage effects. In many of these models, the number of employees was also dropped from the model. A consistent finding is that initial establishment employment is not associated with changes in the hazard rate, but the county high tech wage is, whether or not establishment employment is included. The unemployment rate is significant only when it enters with the high-tech wage because the two variables are negatively correlated and both enter with a positive sign.

To further evaluate the sensitivity of our findings, we also estimate a simple probit model of exit where the vector of exogenous characteristics is defined with respect to the current period, i.e., it is time-varying. This model estimates the probability that the establishment exits following period t as a function of the variable vector $Z_{i,t}$, i.e., the same set of variables as in the Weibull duration model, except that values are not for the establishment's entry period but for the current period. The age of the establishment is also included as a regressor to allow for duration dependence. The results are qualitatively similar to the ones presented above, with only minor exceptions. The probit results do not exhibit significant duration dependence, with the log of age being positive, but not statistically significant. However, the number of employees in the preceding period reduces exit rates, possibly because it reflects current profitability.

In brief, there is no strong spatial association between software publisher activity and software establishment exit rates. There is only limited association between other variables and exit. We discuss the implications of the combination of results in the employment growth (at the location and establishment level), establishment entry, and establishment exit in our concluding section.

5 Interpretation of Results and Concluding Remarks

Having discussed each facet of the econometric analysis separately, we summarize here what the body of evidence suggests about the association between agglomeration in the software industry and the employment dynamics in that industry. In interpreting the body of findings, it is important to keep in mind that the choice of the software industry eliminates most agglomeration or localization economies other than those from human capital spillovers or through the labor market, as argued in the introduction. These sources of spatial affiliation include spin-offs, which can be considered a component of the labor market channel and are primarily relevant for entry rates. Knowledge spillovers and labor market agglomeration effects have a somewhat different "signature," as discussed at length in section 2.2. We will use these differences to assess the relative importance of the two types of spillovers on the basis of our results.

At the smallest level of spatial resolution (within one mile), employment exhibits excess persistence, i.e., greater persistence than one would expect from the employment persistence of individual establishments. This persistence is not driven by the faster employment growth of establishments in areas with multiple software firms. Indeed, incumbent establishments seem to exhibit slower growth when co-located with other establishments. Moreover, the persistence is only partially driven by the fact that a job "lost" by an establishment is "captured" by another establishment in the same location. Rather, the presence of software firms increases the propensity of other software firms to enter within very close proximity. But the establishments entering in localities where other software firms are already present do not experience differential survival rates than establishments entering in localities with no prior software firm activity.

This combination of findings suggests that a prior concentration of software firms in a locality lowers the entry costs of other software firms in that same locality, but that post-entry profitability of the average firm in that locality is not higher and may even be lower (if one focuses on employment growth). Recalling the framework developed in Section 2.2, this pattern is most consistent with spatial effects arising from the localized labor pool, including from localized spin-off effects or firms finding it preferable to locate in a particular location because recruiting is easier. It is hard with our data to disentangle the relative importance of spin-offs. Nevertheless, the descriptive evidence in section 3.3 argues against spin-offs (unless spin-offs enter different market segments than the parent firm).

A competing (or complementary) explanation is that entrant firms co-locate because of synergies or direct human capital spillovers from the incumbent firms in those areas. However, re-examining Panel A of Figure 1 reveals that such spillovers should make the average entrant into that location more successful compared to elsewhere and should also benefit incumbent firms. There is no evidence of this effect at the 1 mile range and, therefore, human capital spillovers cannot be the only source of agglomeration economies in this industry. However, some human capital spillovers might be present in conjunction with the primary labor channel effects. These human capital spillovers would reinforce the positive effects of labor channel spillovers in increasing the post-entry payoff function in high spatial agglomeration locations. Even though lower entry costs in those locations mean that software establishments co-locating with other software firms are on average weaker business prospects, the upward shift in the payoff function would leave average exit rates unchanged (and employment growth of incumbent firms only marginal lower).[46]

Moving to intermediate distances (between 1 and 10 miles), we find there is no strong and consistent relationship between the end-of-period software employment in a location and the software employment in the surrounding area, whether initial or final. However, there is still a positive, but diminished, association between entry rates and prior software firm activity. There continues to be no association between exit rates and software firm activity. In other words, entry costs in a location appear lower if there is a labor pool in the surrounding area, but the surrounding pool does not enhance the productivity of software firms in that location. By facilitating entry, it merely leads to a reshuffling of employees from incumbent firms at the start of the sample period to the entrants. On balance, this evidence supports the labor pool interpretations we suggested in the preceding paragraph.

Finally, at even larger distances (between 10 and 25 miles), employment in a location is decreasing in the contemporaneous employment growth at those distances. This is suggestive of a "pull" effect for employees from proximate high growth areas. Also, there is now only a tenuous positive relationship between entry rates in the location and employment at 10 to 25 miles. This evidence is consistent with labor pool effects.

On net, our findings are suggestive of strong labor pool agglomeration economies and weak productivity spillovers from human capital and knowledge transmission. Firms are attracted to locations because of the existing labor force and the effect is strongest at high levels of spatial resolution. This contributes to employment growth in those locations, as does the ability of firms located in close proximity to absorb jobs lost by other co-located firms. However, firms located in those areas do not appear to grow faster,

[46]Unfortunately, we have no way of directly estimating entry costs or firm payoffs in order to assess the extent of selection that happens due to differential entry rates and the relative contribution, if any, of human capital spillovers. However, we can certainly say that the data is inconsistent with a pure human capital spillover story and it is also inconsistent with a framework mentioned in footnote 7 since under that framework firm post-entry performance improves.

i.e., they do not benefit from the agglomeration in the industry. Indeed, it appears that the more intense competition for employees that results from this entry in areas with prior industry presence yields a more competitive labor input environment, somewhat reduces end-of-period employment at the firm level, and possibly slightly increases the failure rate of the entrants.

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Table 1: Summary statistics for software firms

Panel A: Full sample of firms			
	All	Incumbents	Entrants[a]
Unique number of firms	877	506	371
Number of establishments	1.228	1.106	1.552
per firm	(1.041)	(0.583)	(1.707)
Unique number of establishments	957	526	431
Number of firms with one establishment	857	497	360
Average size (employment)	34.955	31.071	45.256
of establishments	(194.517)	(113.474)	(322.322)
Own quarterly wage ($) per establishment	20,305.82	19,089.65	23,530.49
	(25,229.45)	(20,133.96)	(35,147.05)
Age (in months) per establishment	105.983	129.217	44.379
	(85.166)	(83.681)	(51.886)
Average time in the sample (quarters)	11.7000	13.038	8.150
	(7.6111)	(7.758)	(5.880)
University spillover ($)	3,379,909.00	3,422,106.00	3,268,024.00
	(9,146,528.00)	(9,487,206.00)	(8,174,685.00)
Junior college spillover ($)	16,301.18	18,623.02	10,144.80
	(65,706.70)	(75,875.25)	(21,171.85)
Quarterly average wage ($) rate for	13,653.58	13,433.91	14,236.03
high-tech industries in the county	(3,717.31)	(3,676.62)	(3,761.90)
County unemployment rate	5.385	5.362	5.445
	(1.239)	(1.284)	(1.109)
County amenity LQ	1.065	1.066	1.064
	(0.198)	(0.202)	(0.188)
Panel B: Randomly chosen non-overlapping locations			
	All	At least one incumbent	At least one entrant
Number of unique locations	9,299	201	170
Average number of software	1.478	65.870	68.064
employees: within a mile	(34.464)	(224.785)	(241.105)
Average number of software	33.078	661.003	859.977
employees: 1 - 5 miles	(284.603)	(1,241.156)	(1,400.822)
Average number of other industry	646.724	10,718.010	11,798.320
employees: within a mile	(3,828.118)	(19,788.580)	(21,665.510)
Average number of other industry	11,985.390	138,240.400	144,071.500
employees: 1 - 5 miles	(42,743.660)	(141,583.400)	(145,221.700)
Average number of software	0.001	0.033	0.063
entrants	(0.036)	(0.195)	(0.259)
Average size of other establishments	11.281	24.882	28.495
	(32.011)	(37.7587)	(43.378)
Average quarterly wage ($) other	6,058.15	11,028.08	12,035.130
establishments	(10,669.55)	(5,369.81)	(5,676.845)

Standard deviations are in parentheses.

[a] Entered after Mar 31, 2000

Table 2: Rival information within five miles.

City	Location	Establishment	Category	Entry date	Exit date	Quarterly employment	Distance (in miles)
A	1	1	Comp. software & services.	1990-03	2005-02	91.14	–
		2	Interactive comp. software.	2005-02		141.79	–
	2	3	Comp., peripheral equip.and software.	2000-11		2.44	1.005
	3	4	Business software.	1997-01		39.90	2.030
	4	5	Aircraft performance software.	1996-10		3.01	3.746
	5	6	Develop comp. software.	1977-12		33.56	3.834
B	1	1	Electronic trading software.	1993-12	2005-09	22.722	
	2	2	Investment & banking software.	1983-12		5.890	0.579
		3	Comp. software & services.	1992-04		68.32	0.579
	3	4	Accounting services software.	1996-04	2005-12	3.067	0.587
	4	5	Geophysical and Meteorological software.	1991-03	2005-12	5.967	0.732
	5	6	Healthcare software.	1990-12		46.667	0.943
	6	7	Software engineering.	1993-06		5.528	1.004
C	1	1	Comp. & software related services.	2000-12		3.81	–
	2	2	Comp. software.	1998-04	2005-12	14.58	0.69
		3	Comp. software services.	2006-01		13.08	0.69
	3	4	Comp. consultants.	1999-11		7.88	1.79
	4	5	No information.	1990-09	2004-03	2.00	3.39
	5	6	Industrial control instruments software.	1990-12		3.90	3.41
	6	7	Coommunication systems software.	1992-09		340.85	3.53
	7	8	Systems software development services.	2004-02		16.33	3.84
	8	9	Data management and business software.	1994-03		27.38	3.93
	9	10	Fluid meters & counting devices software.	2001-11		12.31	4.30
	10	11	Customer privacy & protection.	1995-01		652.10	4.51
	11	12	Comp. software services.	2003-01		148.50	4.58
	12	13	Coommunication systems software.	1997-03		5.77	4.64
	13	14	Identity management solutions.	1990-09		5.56	4.75
		15	Engineering & communication software.	1996-01		32.55	4.75
	14	16	Business & engineering software.	1998-10	2004-03	3.08	4.89
D	1	1	Business solutions.	1999-01		7.264	
	2	2	Insurance and business software.	1999-01	2006-06	572.00	0.56
		3	Business intelligence software.	2002-10		598.500	0.56
	3	4	Information technology.	1996-02		2.00	3.17
	4	5	No information	2002-01		7.417	4.86
E	1	1	Entertainment software.	1998–07		18.15	–
		2	Develop comp. games software.	2004-01	2005-09	9.14	–
	2	3	Software for automating data integration.	1991-03		54.75	0.12
	3	4	Entertainment software.	2002-09	2002-12	4.00	0.17
	4	5	Engineering software.	2001-02	2005-03	6.10	0.25
		6	Enterprise network configurations.	2001-10		48.28	0.25
	5	7	Advertising software.	1999-04	2000-12	42.69	0.39
	6	8	Creative design software.	2000-12	2006-06	6.35	0.49
	7	9	Digital solutions software.	1994-07	2006-09	2.28	0.49
	8	10	Programming, analysing, & designing.	1990-09		2.30	0.62

Table 3: Censored log regression results for software employment growth at a location

Variable	Log(*employment*)					
	(1)	(2)	(3)	(4)	(5)	(6)
Log initial period number of software	0.656***	0.599***	0.502***	0.530***	0.483***	0.496***
employees: within a mile	(0.057)	(0.065)	(0.072)	(0.068)	(0.087)	(0.082)
Log initial period number of software		0.062	-0.010	0.162*	0.006	0.163
employees: 1 - 5 miles		(0.052)	(0.054)	(0.090)	(0.121)	(0.135)
Log initial period number of software		-0.141	-0.164*	-0.116	-0.545**	-0.359
employees: 5 - 10 miles		(0.088)	(0.085)	(0.163)	(0.214)	(0.230)
Log initial period number of software		0.116	0.101	0.428***	-0.044	0.221
employees: 10 - 25 miles		(0.076)	(0.075)	(0.162)	(0.130)	(0.191)
Log initial period weighted number of other			0.118	0.141	0.126	0.187
industry employees: within a mile			(0.172)	(0.188)	(0.182)	(0.194)
Log initial period number of software					0.467	0.606*
firms: within a mile					(0.341)	(0.332)
Log initial period number of software					-0.142	0.258
firms: 1 - 5 miles					(0.298)	(0.504)
Log initial period number of software					0.788**	1.058*
firms: 5 - 10 miles					(0.401)	(0.543)
Log initial period number of software					0.238	0.972*
firms: 10 - 25 miles					(0.317)	(0.578)
Log final period number of rival software						-0.226
firms: 1 - 5 miles						(0.439)
Log final period number of rival software						-0.520
firms: 5 - 10 miles						(0.632)
Log final period number of rival software						-0.303
firms: 10 – 25 miles						(0.608)
Rivals' log final period number of software				-0.218**		-0.264*
employees: 1 – 5 miles				(0.087)		(0.139)
Rivals' log final period number of software				-0.055		-0.123
employees: 5 – 10 miles				(0.147)		(0.204)
Rivals' log final period number of software				-0.397**		-0.455**
employees: 10 – 25 miles				(0.168)		(0.221)
Log final period weighted number of other			0.125	0.138	0.073	0.046
industry employees: within a mile			(0.189)	(0.204)	(0.207)	(0.211)
Initial period variables		Yes**	Yes**	Yes**	Yes**	Yes**
Final period variables				Yes**	Yes**	Yes**
Major county effects	Yes**	Yes**	Yes**	Yes**	Yes**	Yes**
Number of obs.	201	201	201	201	201	201
Number of uncensored obs.	150	150	150	150	150	150
Log likelihood	-350.207	-337.907	-331.673	-325.971	-326.747	-320.076

Table reports marginal effects on the expected values of log employment, associated robust standard errors are in parentheses. *** denotes statistical significance at the 1 percent level, ** denotes statistical significance at the 5 percentlevel and * denotes statistical significance at the 10 percent level. The dependent variable is the log of total number of employees in non-overlapping locations in the last period. Initial and final period variables include spillovers, unemployment rate, and county high-tech wages. Final period variables are lagged by one period. See text for details.

Table 4: Censored linear regression results for software employment growth at a location

Variable	*Employment*					
	(1)	(2)	(3)	(4)	(5)	(6)
Initial period number of software	0.753***	0.739***	0.696***	0.693***	0.645***	0.617***
employees: within a mile	(0.199)	(0.186)	(0.161)	(0.156)	(0.137)	(0.131)
Initial period number of software		0.011	0.004	0.132*	-0.030	0.117*
employees: 1 - 5 miles		(0.017)	(0.016)	(0.068)	(0.041)	(0.061)
Initial period number of software		-0.024	-0.024	0.065*	-0.048	0.059
employees: 5 - 10 miles		(0.022)	(0.022)	(0.037)	(0.036)	(0.042)
Initial period number of software		0.031	0.031	0.096	0.019	0.083**
employees: 10 - 25 miles		(0.029)	(0.030)	(0.058)	(0.016)	(0.042)
Initial period weighted number of other			-0.026	-0.008	-0.003	0.036
industry employees: within a mile			(0.062)	(0.068)	(0.062)	(0.073)
Initial period number of software					33.519**	44.492***
firms: within a mile					(14.200)	(17.089)
Initial period number of software					1.760	6.003
firms: 1 - 5 miles					(2.477)	(7.997)
Initial period number of software					0.815	7.126*
firms: 5 - 10 miles					(1.800)	(4.124)
Initial period number of software					1.173	-1.137
firms: 10 - 25 miles					(2.132)	(3.626)
Final period number of rival software						0.796
firms: 1 - 5 miles						(6.219)
Final period number of rival software						-5.417
firms: 5 - 10 miles						(4.186)
Final period number of rival software						6.102
firms: 10 – 25 miles						(5.057)
Rivals' final period number of software				-0.119*		-0.206**
employees: 1 – 5 miles				(0.064)		(0.103)
Rivals' final period number of software				-0.079*		-0.110*
employees: 5 – 10 miles				(0.043)		(0.066)
Rivals' final period number of software				-0.058*		-0.102*
employees: 10 – 25 miles				(0.035)		(0.057)
Weighted number of other industry			0.057	0.055	0.014	-0.013
employees: within a mile			(0.036)	(0.038)	(0.040)	(0.051)
Initial period variables		Yes**	Yes**	Yes**	Yes**	Yes**
Final period variables				Yes**	Yes**	Yes**
Major county effects	Yes**	Yes**	Yes**	Yes**	Yes**	Yes**
Number of obs.	201	201	201	201	201	201
Number of uncensored obs.	150	150	150	150	150	150
Log likelihood	-1,081.353	-1,074.429	-1,072.3045	-1,066.539	-1,069.064	-1,055.148

Table reports weighted marginal effects (weighted by the initial employment at the location) on the expected value of final employment, associated robust standard errors are in parentheses. *** denotes statistical significance at the 1 percent level, ** denotes statistical significance at the 5 percentlevel and * denotes statistical significance at the 10 percent level. The dependent variable is the total number of employees in non-overlapping locations in the last period. periodInitial and final variables include spillovers, unemployment rate, and county high-tech wages. Final period variables are lagged by one period. See text for details.

Table 5: Censored log regression results for software establishment–level employment growth

Variable	Establishment-level log(*employment*)							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Log of initial period establishment-level employment	0.366***	0.363***	0.347***	0.351***	0.402***	0.399***	0.391***	0.408***
	(0.041)	(0.042)	(0.043)	(0.043)	(0.046)	(0.046)	(0.046)	(0.049)
Log initial period number of other software employees: within a mile					-0.069*	-0.077*	-0.124***	-0.184**
					(0.036)	(0.042)	(0.046)	(0.073)
Log initial period number of other software employees: 1 - 5 miles		-0.053	-0.076*	-0.016		-0.022	-0.045	-0.080
		(0.038)	(0.041)	(0.101)		(0.042)	(0.043)	(0.104)
Log initial period number of other software employees: 5 - 10 miles		0.084	0.087	0.119		0.074	0.073	0.075
		(0.056)	(0.057)	(0.134)		(0.056)	(0.056)	(0.135)
Log initial period number of other software employees: 10 - 25 miles		-0.058	-0.048	-0.037		-0.065	-0.050	-0.072
		(0.053)	(0.053)	(0.174)		(0.052)	(0.053)	(0.173)
Log initial period weighted number of other industry employees: within a mile			0.057	0.079*			0.105**	0.103**
			(0.039)	(0.042)			(0.043)	(0.043)
Log initial period number of other software firms: within a mile				-0.128				0.151
				(0.107)				(0.154)
Log initial period number of other software firms: 1 - 5 miles				-0.069				0.056
				(0.210)				(0.215)
Log initial period number of other software firms: 5 - 10 miles				-0.076				-0.022
				(0.266)				(0.266)
Log initial period number of other software firms: 10 - 25 miles				-0.014				0.049
				(0.283)				(0.283)
Initial period variables		Yes**	Yes**	Yes**	Yes**	Yes**	Yes**	Yes**
Major county effects	Yes**	Yes**	Yes**	Yes**	Yes**	Yes**	Yes**	Yes**
Number of obs.	528	528	528	528	528	528	528	528
Number of uncensored obs.	268	268	268	268	268	268	268	268
Log likelihood	-792.693	-789.586	-788.534	-787.477	-790.567	-787.887	-784.896	-784.313

Table reports marginal effects on the expected values of log employment, associated robust standard errors are in parentheses. *** denotes statistical significance at the 1 percent level, ** denotes statistical significance at the 5 percentlevel and * denotes statistical significance at the 10 percent level. The dependent variable is the log of total number of employees in an establishment in the last period. Initial period variables include spillovers, unemployment rate, and county high-tech wages. See text for details.

Table 6: Censored linear regression results for software establishment–level employment growth

Variable	*Establishment-level employment*							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Initial period establishment-level employment	0.283***	0.285***	0.284***	0.289***	0.290***	0.293***	0.294***	0.297**
	(0.024)	(0.024)	(0.024)	(0.024)	(0.025)	(0.025)	(0.025)	(0.025
Initial period number of other software employees: within a mile					-0.008	-0.010	-0.013*	-0.015
					(0.007)	(0.007)	(0.008)	(0.010
Initial period number of other software employees: 1 - 5 miles		0.001	0.001	0.009		0.002	0.001	0.003
		(0.003)	(0.003)	(0.006)		(0.003)	(0.003)	(0.007
Initial period number of other software employees: 5 - 10 miles		-0.001	-0.001	0.001		-0.001	-0.001	-0.002
		(0.003)	(0.003)	(0.006)		(0.003)	(0.003)	(0.006
Initial period number of other software employees: 10 - 25 miles		0.000	0.000	-0.003		-0.000	-0.000	-0.005
		(0.002)	(0.002)	(0.006)		(0.002)	(0.002)	(0.006
Initial period weighted number of other industry employees: within a mile			0.003	0.003			0.004	0.004
			(0.003)	(0.003)			(0.003)	(0.003
Initial period number of other software firms: within a mile				0.091				0.530
				(0.502)				(0.587
Initial period number of other software firms: 1 - 5 miles				-0.461				-0.207
				(0.288)				(0.338
Initial period number of other software firms: 5 - 10 miles				-0.068				0.063
				(0.279)				(0.293
Initial period number of other software firms: 10 - 25 miles				0.153				0.207
				(0.222)				(0.225
Initial variables		Yes**	Yes**	Yes**	Yes**	Yes**	Yes**	Yes**
Major county effects	Yes**	Yes**	Yes**	Yes**	Yes**	Yes**	Yes**	Yes**
Number of obs.	528	528	528	528	528	528	528	528
Number of uncensored obs.	268	268	268	268	268	268	268	268
Log likelihood	-1706.844	-1705.130	-1704.735	-1703.165	-1706.057	-1704.110	-1,703.278	-1,702.1

Table reports weighted marginal effects (weighted by the initial employment at the location) on the expected value of final employment, associated robust standard errors are in parentheses. *** denotes statistical significance at the 1percent level, ** denotes statistical significance at the 5 percentlevel and * denotes statistical significance at the 10 percent level. The dependent variable is the total number of employees in an establishment in the last period. Initial period variables include spillovers, unemployment rate, and county high-tech wages. See text for details.

Table 7: Censored log regression results for software employment growth of incumbents at a location

Variable	Log(*employment*)					
	(1)	(2)	(3)	(4)	(5)	(6)
Log initial period number of software employees: within a mile	0.528***	0.540***	0.479***	0.492***	0.427***	0.423***
	(0.066)	(0.066)	(0.074)	(0.073)	(0.088)	(0.088)
Log initial period number of software employees: 1 - 5 miles		0.017	-0.029	0.092	0.021	0.094
		(0.051)	(0.054)	(0.090)	(0.121)	(0.140)
Log initial period number of software employees: 5 - 10 miles		-0.152*	-0.164**	-0.200	-0.368*	-0.329
		(0.084)	(0.082)	(0.160)	(0.207)	(0.220)
Log initial period number of software employees: 10 - 25 miles		0.107	0.092	0.186	-0.101	0.041
		(0.072)	(0.071)	(0.152)	(0.121)	(0.160)
Log initial period weighted number of other industry employees: within a mile			-0.099	-0.109	-0.081	-0.046
			(0.170)	(0.179)	(0.180)	(0.184)
Log initial period number of software firms: within a mile					0.745**	0.888***
					(0.339)	(0.336)
Log initial period number of software firms: 1 - 5 miles					-0.185	0.366
					(0.291)	(0.506)
Log initial period number of software firms: 5 - 10 miles					0.333	0.647
					(0.380)	(0.504)
Log initial period number of software firms: 10 - 25 miles					0.371	0.604
					(0.294)	(0.478)
Log final period number of rival software firms: 1 - 5 miles						-0.402
						(0.415)
Log final period number of rival software firms: 5 - 10 miles						-0.554
						(0.581)
Log final period number of rival software firms: 10 – 25 miles						0.217
						(0.560)
Rivals' Log final period number of software employees: 1 – 5 miles				-0.163*		-0.167
				(0.085)		(0.141)
Rivals' Log final period number of software employees: 5 – 10 miles				-0.001		0.016
				(0.144)		(0.195)
Rivals' Log final period number of software employees: 10 – 25 miles				-0.143		-0.309
				(0.150)		(0.208)
Log final period weighted number of other industry employees: within a mile			0.238	0.284	0.170	0.174
			(0.183)	(0.194)	(0.201)	(0.197)
Initial period variables		Yes**	Yes**	Yes**	Yes**	Yes**
Final period variables				Yes**	Yes**	Yes**
Major county effects	Yes**	Yes**	Yes**	Yes**	Yes**	Yes**
Number of obs.	201	201	201	201	201	201
Number of uncensored obs.	136	136	136	136	136	136
Log likelihood	-330.735	-326.026	-324.005	-318.980	-316.960	-312.921

Table reports marginal effects on the expected values of log employment, associated robust standard errors are in parentheses. *** denotes statistical significance at the 1 percent level, ** denotes statistical significance at the 5 percentlevel and * denotes statistical significance at the 10 percent level. The dependent variable is the log of total number of employees in non-overlapping locations in the last period. Initial and final period variables include spillovers, unemployment rate, and county high-tech wages. Final period variables are lagged by one period.
See text for details.

Table 8: Censored linear regression results for software employment growth of incumbents at a location

Variable	*Employment*					
	(1)	(2)	(3)	(4)	(5)	(6)
Initial period number of software	0.539**	0.541**	0.502**	0.499**	0.444**	0.431**
employees: within a mile	(0.240)	(0.229)	(0.214)	(0.208)	(0.187)	(0.183)
Initial period number of software		0.004	-0.005	0.103*	-0.045	0.088
employees: 1 - 5 miles		(0.019)	(0.018)	(0.061)	(0.040)	(0.055)
Initial period number of software		-0.027	-0.025	0.048	-0.034	0.062
employees: 5 - 10 miles		(0.021)	(0.021)	(0.034)	(0.030)	(0.042)
Initial period number of software		0.032	0.034	0.085	0.021	0.081**
employees: 10 - 25 miles		(0.027)	(0.028)	(0.054)	(0.016)	(0.040)
Initial period weighted number of other			-0.097*	-0.087	-0.078	-0.045
industry employees: within a mile			(0.057)	(0.058)	(0.055)	(0.060)
Initial period number of software					40.267**	48.936**
firms: within a mile					(17.128)	(19.702)
Initial period number of software					2.401	8.239
firms: 1 - 5 miles					(2.319)	(7.534)
Initial period number of software					-0.753	5.671
firms: 5 - 10 miles					(1.657)	(4.056)
Initial period number of software					1.533	0.319
firms: 10 - 25 miles					(2.137)	(3.800)
Final period number of rival software						-1.770
firms: 1 - 5 miles						(5.853)
Final period number of rival software						-5.846
firms: 5 - 10 miles						(4.612)
Final period number of rival software						4.272
firms: 10 – 25 miles						(4.766)
Rivals' final period number of software				-0.103*		-0.175*
employees: 1 – 5 miles				(0.059)		(0.094)
Rivals' final period number of software				-0.066*		-0.090
employees: 5 – 10 miles				(0.038)		(0.058)
Rivals' final period number of software				-0.046		-0.084
employees: 10 – 25 miles				(0.032)		(0.052)
Weighted number of other industry			0.089**	0.089***	0.041	0.018
employees: within a mile			(0.037)	(0.034)	(0.035)	(0.045)
Initial period variables		Yes**	Yes**	Yes**	Yes**	Yes**
Final period variables				Yes**	Yes**	Yes**
Major county effects	Yes**	Yes**	Yes**	Yes**	Yes**	Yes**
Number of obs.	201	201	201	201	201	201
Number of uncensored obs.	136	136	136	136	136	136
Log likelihood	-994.816	-987.977	-986.327	-981.557	-981.465	-970.917

Table reports weighted marginal effects (weighted by the initial employment at the location) on the expected value of final employment, associated robust standard errors are in parentheses. ***denotes statistical significance at the 1 percent level, ** denotes statistical significance at the 5 percent level and * denotes statistical significance at the 10 percent level. The dependent variable is the total number of employees in non-overlapping locations in the last period. Initial and final period variables include spillovers, unemployment rate, and county high-tech wages. Final period variables are lagged by one period. See text for details.

Table 9: Ordered probit regression results for software establishment entry

Variable	Number of new software entrants						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Log of lagged number of software	0.372***	0.328***	0.352***				0.264***
firms: within a mile	(0.047)	(0.049)	(0.048)				(0.088)
Log of lagged number of software	0.205***	0.192***	0.178***				0.015
firms: 1 – 5 miles	(0.042)	(0.043)	(0.042)				(0.071)
Log of lagged number of software	0.041	0.040	0.020				0.217**
firms: 5 – 10 miles	(0.048)	(0.049)	(0.048)				(0.097)
Log of lagged number of software	0.083**	0.095***	0.010				-0.105
firms: 10 – 25 miles	(0.034)	(0.035)	(0.035)				(0.072)
Log of lagged number of software				0.119***	0.104***	0.111***	0.035
employees: within a mile				(0.015)	(0.015)	(0.015)	(0.029)
Log of lagged number of software				0.111***	0.100***	0.100***	0.077***
employees: 1 – 5 miles				(0.016)	(0.017)	(0.016)	(0.029)
Log of lagged number of software				0.001	0.002	-0.016	-0.099**
employees: 5 – 10 miles				(0.019)	(0.019)	(0.019)	(0.044)
Log of lagged number of software				0.058***	0.060***	0.025	0.060*
employees: 10 – 25 miles				(0.015)	(0.015)	(0.016)	(0.035)
Log of lagged weighted number of other	0.119***	0.143***	0.127***	0.127***	0.152***	0.134***	0.123***
industry employees: within a mile	(0.017)	(0.018)	(0.017)	(0.016)	(0.017)	(0.017)	(0.018)
Log oflagged university spillover			0.021			0.023*	0.019
			(0.013)			(0.013)	(0.013)
Log of lagged junior college spillover			-0.027*			-0.027*	-0.024
			(0.016)			(0.016)	(0.016)
Log of lagged average wage of high-tech			0.753***			0.774***	0.763***
industries in the county			(0.127)			(0.124)	(0.128)
Lagged county unemployment rate			-0.078***			-0.073***	-0.076***
			(0.028)			(0.028)	(0.028)
Lagged county amenity LQ			-0.009			-0.051	-0.025
			(0.107)			(0.115)	(0.110)
Trend	-0.359	-0.382	0.458	-0.167	-0.193	0.519	0.463
	(0.339)	(0.341)	(0.491)	(0.339)	(0.341)	(0.493)	(0.495)
Trend^2	-0.049	-0.045	-0.928**	-0.252	-0.241	-1.003**	-0.946**
	(0.332)	(0.334)	(0.464)	(0.331)	(0.333)	(0.466)	(0.468)
Top 5 county effects		Yes**			Yes**		
Thresholds							
μ_1	3.651***	3.683***	10.045***	3.733***	3.749***	10.300***	10.148***
	(0.084)	(0.086)	(1.139)	(0.086)	(0.088)	(1.120)	(1.150)
μ_2	4.954***	5.002***	11.382***	5.030***	5.061***	11.630***	11.490***
	(0.138)	(0.140)	(1.149)	(0.139)	(0.141)	(1.129)	(1.159)
Number of obs.	251,073	251,073	251,073	251,073	251,073	251,073	251,073
Log likelihood	-1,408.115	-1,394.585	-1,383.135	-1,412.280	-1,401.223	-1,386.207	-1376.097
LR χ^2	1,616.660	1,643.710	1,666.620	1,608.330	1,630.4440	1,660.470	1680.690
Pseudo R^2	0.365	0.371	0.376	0.363	0.368	0.375	0.379

*** denotes statistical significance at the 1 percent level, ** denotes statistical significance at the 5 percent level and * denotes statistical significance at the 10 percent level. Robust standard errors are in parentheses. The dependent variable takes the value 0 for no entrants, 1 for one entrant, and 2 for two or more entrants at a given location. See text for details.

Table 10: Poisson regression results for software establishment entry

Variable	Number of new software entrants						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Log of lagged number of software	0.622***	0.625***	0.663***				0.468**
firms: within a mile	(0.119)	(0.118)	(0.115)				(0.210)
Log of lagged number of software	0.545***	0.501***	0.460***				-0.003
firms: 1 – 5 miles	(0.122)	(0.120)	(0.124)				(0.196)
Log of lagged number of software	0.190	0.167	0.094				0.504**
firms: 5 – 10 miles	(0.151)	(0.146)	(0.148)				(0.253)
Log of lagged number of software	0.352***	0.166	0.092				-0.158
firms: 10 – 25 miles	(0.107)	(0.107)	(0.110)				(0.197)
Log of lagged number of software				0.204***	0.204***	0.212***	0.082
employees: within a mile				(0.034)	(0.033)	(0.031)	(0.064)
Log of lagged number of software				0.285***	0.269***	0.270***	0.227***
employees: 1 – 5 miles				(0.046)	(0.047)	(0.049)	(0.083)
Log of lagged number of software				0.062	0.035	-0.006	-0.203*
employees: 5 – 10 miles				(0.053)	(0.053)	(0.054)	(0.115)
Log of lagged number of software				0.174***	0.096**	0.073	0.123
employees: 10 – 25 miles				(0.046)	(0.045)	(0.047)	(0.086)
Log of lagged weighted number of other	0.371***	0.363***	0.331***	0.393***	0.385***	0.348***	0.316***
industry employees: within a mile	(0.049)	(0.049)	(0.049)	(0.044)	(0.045)	(0.044)	(0.051)
Log of lagged university spillover		0.011	0.087***		0.019	0.075**	0.072**
		(0.043)	(0.033)		(0.041)	(0.033)	(0.033)
Log of lagged junior college spillover		-0.088*	-0.095**		-0.074	-0.084*	-0.078
		(0.052)	(0.048)		(0.052)	(0.048)	(0.049)
Log of lagged average wage of high-tech		3.165***	2.367***		3.230***	2.452***	2.421***
industries in the county		(0.290)	(0.331)		(0.288)	(0.327)	(0.328)
Lagged county unemployment rate		0.056	-0.206***		0.051	-0.184**	-0.193**
		(0.075)	(0.074)		(0.074)	(0.074)	(0.075)
Lagged county amenity LQ		0.174	-0.114		0.119	-0.251	-0.169
		(0.290)	(0.289)		(0.296)	(0.317)	(0.304)
Trend	-1.227	-2.237	1.628	-0.516	-1.648	1.792	1.568
	(0.839)	(1.391)	(1.417)	(0.839)	(1.375)	(1.418)	(1.418)
Trend^2	0.125	0.319	-2.799**	-0.571	-0.208	-2.999**	-2.803**
	(0.800)	(1.262)	(1.286)	(0.797)	(1.247)	(1.286)	(1.284)
Top 5 county effects	Yes**	Yes**		Yes**	Yes**		
Number of obs.	251,073	251,073	251,073	251,073	251,073	251,073	251,073
Log likelihood	-1,416.075	-1,377.211	-1,396.802	-1,423.232	-1,382.315	-1,397.364	-1,390.025

*** denotes statistical significance at the 1 percent level, ** denotes statistical significance at the 5 percent level and * denotes statistical significance at the 10 percent level. Robust standard errors are in parentheses. The dependent variable is the number of new software entrants. All models are estimated using STATA's ppml routine with keep option. See text for details.

Table 11: Software establishment survival estimates

Variable	Hazard rate determinants						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Log initial number of rival software	0.013	-0.006			-0.264	-0.267	-0.212
firms: within a mile	(0.137)	(0.137)			(0.210)	(0.209)	(0.215)
Log initial number of rival software	-0.089	-0.086			-0.036	-0.038	-0.119
firms: 1 – 5 mile	(0.137)	(0.136)			(0.263)	(0.263)	(0.270)
Log initial number of rival software	0.170	0.171			0.050	0.047	0.017
firms: 5 – 10 mile	(0.151)	(0.151)			(0.289)	(0.288)	(0.294)
Log initial number of rival software	-0.018	-0.021			-0.156	-0.158	-0.263
firms: 10 – 25 mile	(0.101)	(0.103)			(0.285)	(0.286)	(0.294)
Rivals' log initial number of software			0.032	0.053	0.127*	0.127*	0.105
employees: within a mile			(0.048)	(0.048)	(0.072)	(0.072)	(0.075)
Rivals' log initial number of software			-0.045	-0.045	-0.030	-0.029	0.001
employees: 1 – 5 mile			(0.058)	(0.058)	(0.118)	(0.118)	(0.121)
Rivals' log initial number of software			0.066	0.061	0.055	0.056	0.067
employees: 5 – 10 mile			(0.065)	(0.065)	(0.129)	(0.128)	(0.129)
Rivals' log initial number of software			0.011	0.015	0.098	0.099	0.129
employees: 10 – 25 mile			(0.063)	(0.063)	(0.172)	(0.172)	(0.175)
Log of initial weighted number of other	-0.030	-0.004	-0.044	-0.031	-0.006	-0.005	-0.007
industry employees: within a mile	(0.060)	(0.060)	(0.057)	(0.056)	(0.060)	(0.060)	(0.060)
Log of initial university spillover	0.009	0.006	0.012	0.009	-0.008	-0.007	-0.014
	(0.061)	(0.061)	(0.062)	(0.062)	(0.062)	(0.062)	(0.055)
Log of initial junior college spillover	0.018	0.015	0.015	0.011	0.011	0.012	0.022
	(0.046)	(0.046)	(0.047)	(0.047)	(0.047)	(0.046)	(0.045)
Log initial number of employees		-0.101		-0.113	-0.150*	-0.151*	-0.132*
		(0.074)		(0.075)	(0.079)	(0.080)	(0.080)
Log of initial quarterly own wage	-0.033	-0.015	-0.041	-0.029	-0.017		
	(0.093)	(0.090)	(0.093)	(0.090)	(0.091)		
Log of initial average wage of high-tech							1.585***
industries in the county							(0.581)
Initial county unemployment rate	0.123	0.130	0.129	0.136	0.148	0.150	0.264**
	(0.119)	(0.119)	(0.118)	(0.118)	(0.119)	(0.119)	(0.124)
Initial county amenity LQ	-1.208**	-1.203**	-1.280**	-1.321**	-1.316**	-1.304***	-1.473***
	(0.513)	(0.504)	(0.524)	(0.518)	(0.517)	(0.506)	(0.514)
Initial trend & initial trend^2	Yes**	Yes**	Yes**	Yes**	Yes**	Yes**	Yes**
Top 5 county effects	Yes**	Yes**	Yes**	Yes**	Yes**	Yes**	Yes**
Hazard rate: $\ln(p)$	0.361***	0.366***	0.361***	0.367***	0.369***	0.369***	0.387***
	(0.063)	(0.062)	(0.063)	(0.064)	(0.063)	(0.063)	(0.064)
Number of obs.	423	423	423	423	423	423	423
Log likelihood	-395.528	-394.228	-395.191	-393.623	-392.456	-392.474	-388.771
Wald χ^2	35.790	37.290	36.260	38.150	40.780	40.750	53.750

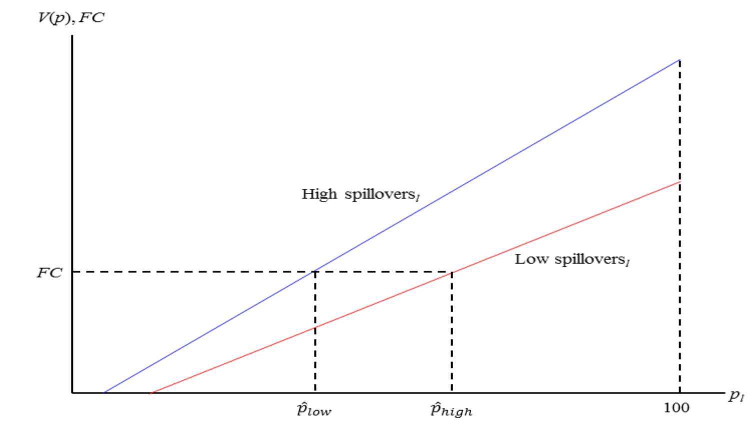
*** denotes statistical significance at the 1 percent level, ** denotes statistical significance at the 5 percent level and * denotes statistical significance at the 10 percent level. Robust standard errors are in parentheses. See text for details.

Table 12: Software establishment survival estimates: sensitivity analysis

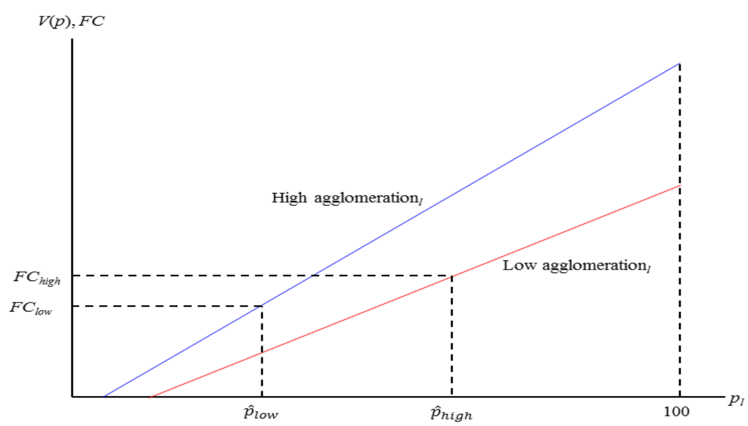
Variable	Hazard rate determinants							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Log initial number of rival software firms: within a mile	0.007	-0.009			0.008	-0.006		
	(0.136)	(0.136)			(0.137)	(0.137)		
Log initial number of rival software firms: 1 – 5 miles	-0.089	-0.086			-0.115	-0.110		
	(0.137)	(0.136)			(0.133)	(0.132)		
Log initial number of rival software firms: 5 – 10 miles	0.167	0.169			0.157	0.157		
	(0.151)	(0.151)			(0.151)	(0.150)		
Log initial number of rival software firms: 10 – 25 miles	-0.019	-0.022			-0.084	-0.079		
	(0.102)	(0.103)			(0.104)	(0.105)		
Rivals' log initial number of software employees: within a mile			0.029	0.051			0.020	0.039
			(0.048)	(0.048)			(0.049)	(0.048)
Rivals' log initial number of software employees: 1 – 10 miles			-0.045	-0.046			-0.046	-0.046
			(0.058)	(0.058)			(0.056)	(0.056)
Rivals' log initial number of software employees: 1 – 10 miles			0.065	0.059			0.051	0.047
			(0.065)	(0.065)			(0.064)	(0.065)
Rivals' log initial number of software employees: 10 – 25 miles			0.010	0.014			-0.023	-0.015
			(0.062)	(0.063)			(0.063)	(0.064)
Log of initial weighted number of other industry employees: within a mile	0.008	0.027	-0.001	0.007	0.009	0.025	0.001	0.009
	(0.057)	(0.057)	(0.054)	(0.053)	(0.056)	(0.056)	(0.053)	(0.052)
Log of initial university spillover	0.010	0.007	0.013	0.010	0.002	-0.001	0.005	0.002
	(0.061)	(0.061)	(0.062)	(0.062)	(0.054)	(0.053)	(0.055)	(0.055)
Log of initial junior college spillover	0.021	0.017	0.019	0.014	0.031	0.027	0.026	0.022
	(0.045)	(0.045)	(0.046)	(0.046)	(0.044)	(0.045)	(0.045)	(0.045)
Log initial number of employees		-0.102		-0.114		-0.089		-0.098
		(0.075)		(0.076)		(0.075)		(0.076)
Log initial average wage of high-tech industries in the county					1.722***	1.651***	1.645***	1.556***
					(0.567)	(0.565)	(0.564)	(0.565)
Initial county unemployment rate	0.126	0.131	0.133	0.139	0.247**	0.250**	0.254**	0.257**
	(0.118)	(0.118)	(0.117)	(0.117)	(0.123)	(0.123)	(0.124)	(0.124)
Initial county amenity LQ	-1.185**	-1.193**	-1.249**	-1.300**	-1.393***	-1.393***	-1.468***	-1.508***
	(0.501)	(0.493)	(0.513)	(0.507)	(0.511)	(0.503)	(0.519)	(0.513)
Initial trend & initial trend^2	Yes**	Yes**	Yes**	Yes**	Yes**	Yes**	Yes**	Yes**
Top 5 county effects	Yes**	Yes**	Yes**	Yes**	Yes**	Yes**	Yes**	Yes**
Hazard rate: $\ln(p)$	0.361***	0.366***	0.361***	0.367***	0.382***	0.385***	0.381***	0.384***
	(0.062)	(0.063)	(0.062)	(0.063)	(0.063)	(0.064)	(0.063)	(0.064)
Number of obs.	423	423	423	423	423	423	423	423
Log likelihood	-395.594	-394.242	-395.292	-393.673	-391.093	-390.073	-391.121	-389.943
Wald χ^2	35.630	37.230	36.060	38.090	48.670	50.510	48.220	49.910

*** denotes statistical significance at the 1 percent level, ** denotes statistical significance at the 5 percent level and * denotes statistical significance at the 10 percent level. Robust standard errors are in parentheses. See text for details.

Figure 1: Profitability and entry equilibrium in high and low spillover locations

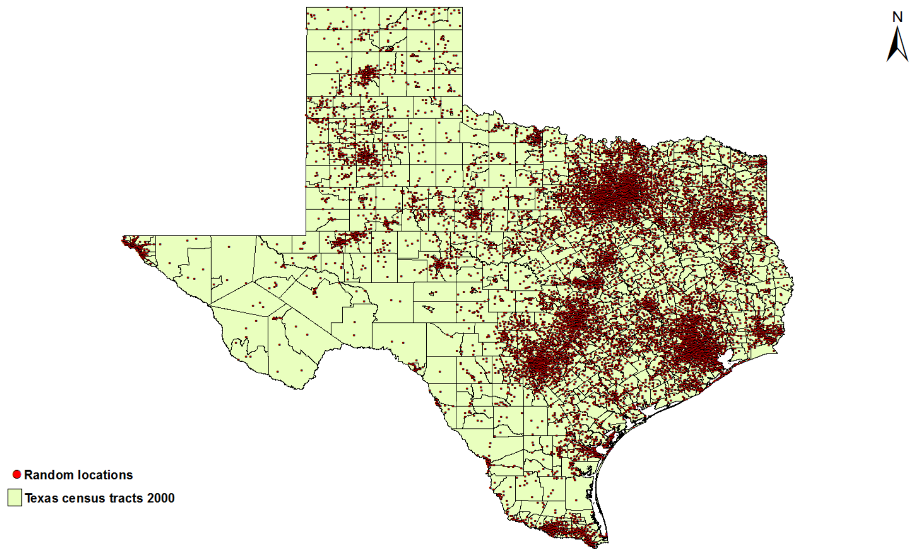


Panel A: High and low knowledge spillover locations

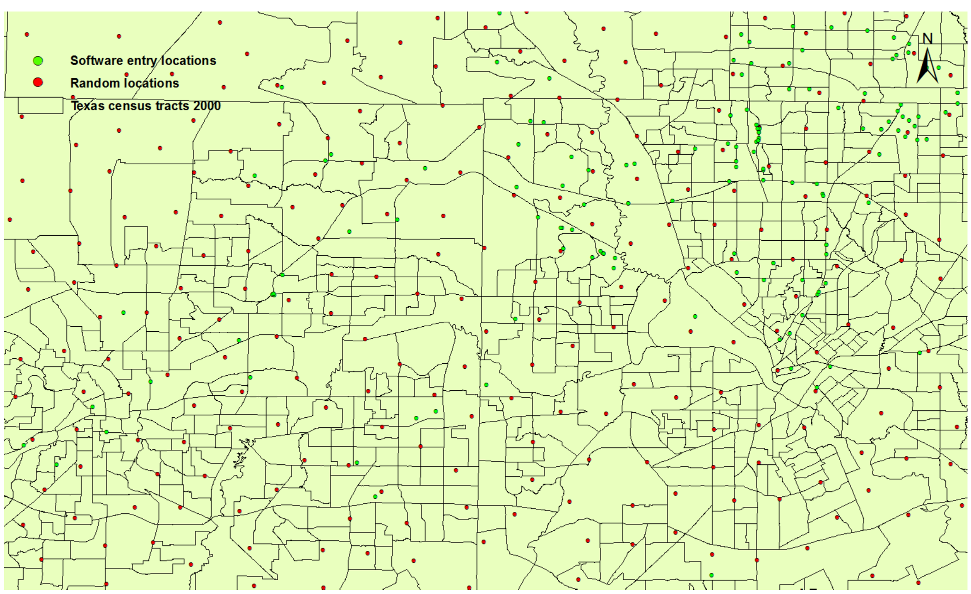


Panel B: High and low labor market agglomeration economies

Figure 2: Non-overlapping locations

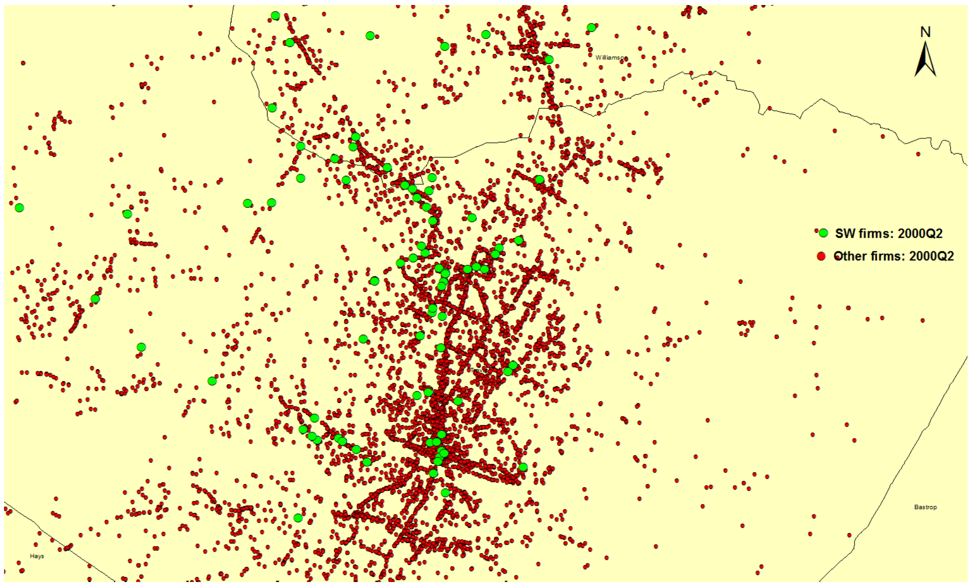


Panel A: Texas

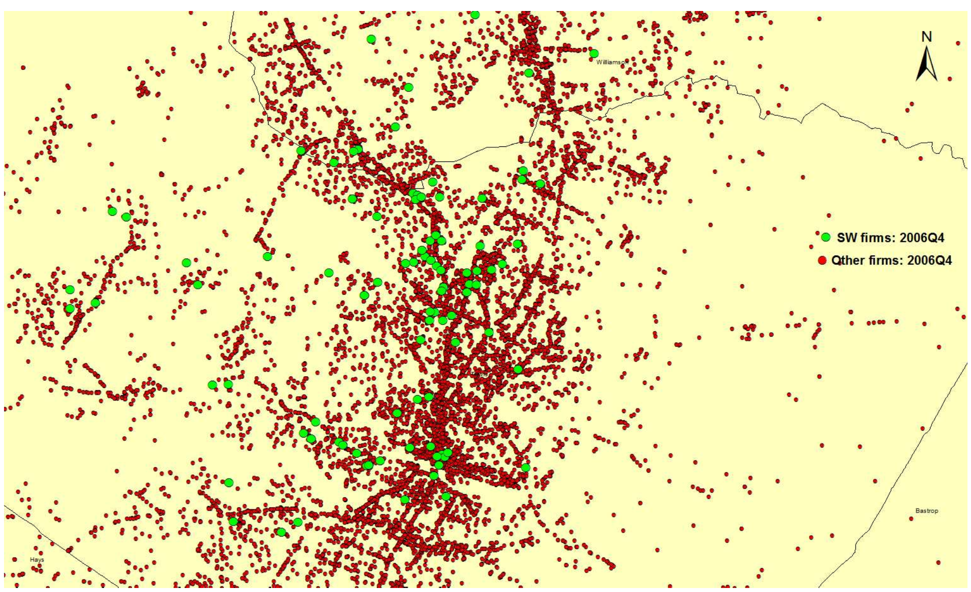


Panel B: Dallas – Fort Worth

Figure 3: Distribution of Software and Other Firms in Austin

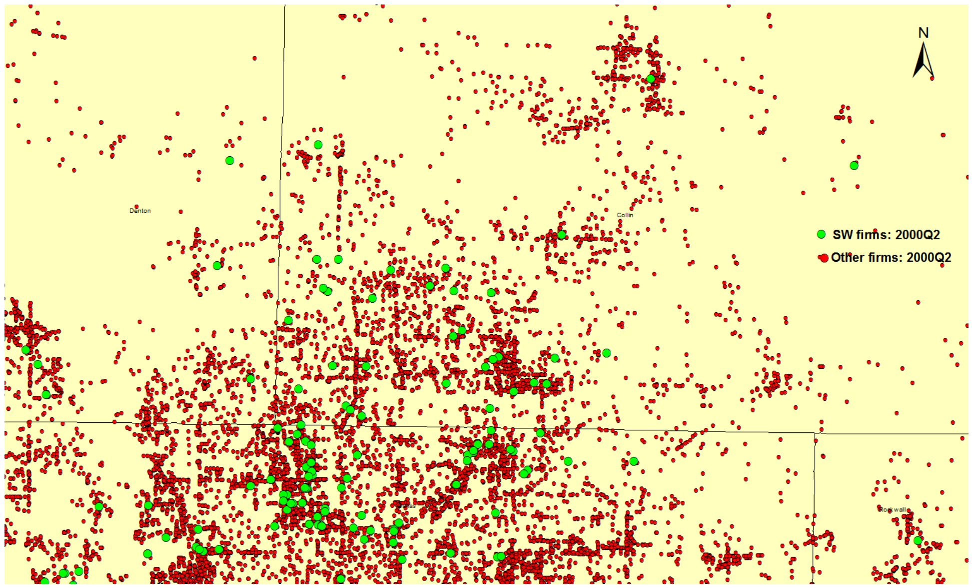


Panel A: 2000 Q2

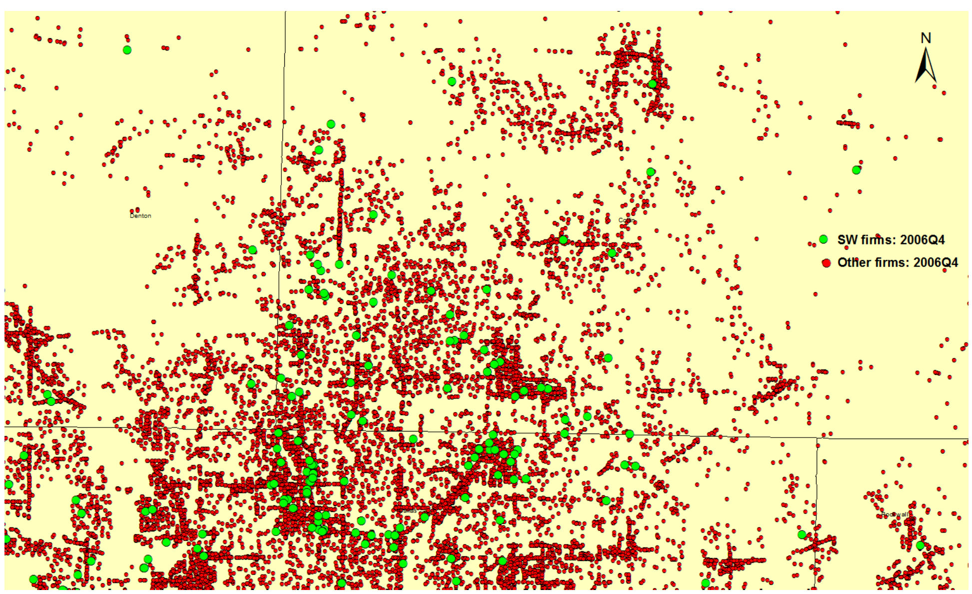


Panel B: 2006 Q4

Figure 4: Distribution of Software and Other Firms in North Dallas

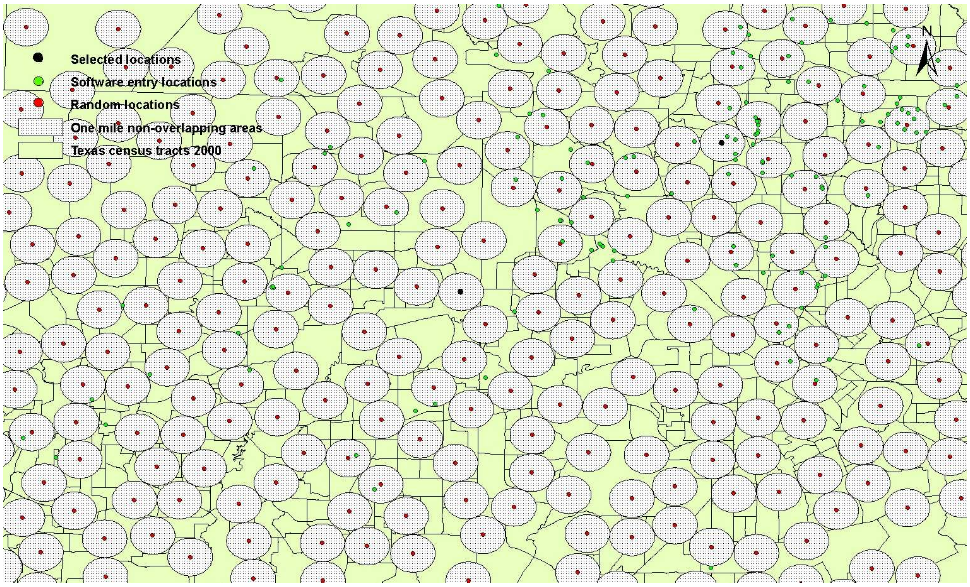


Panel A: 2000 Q2

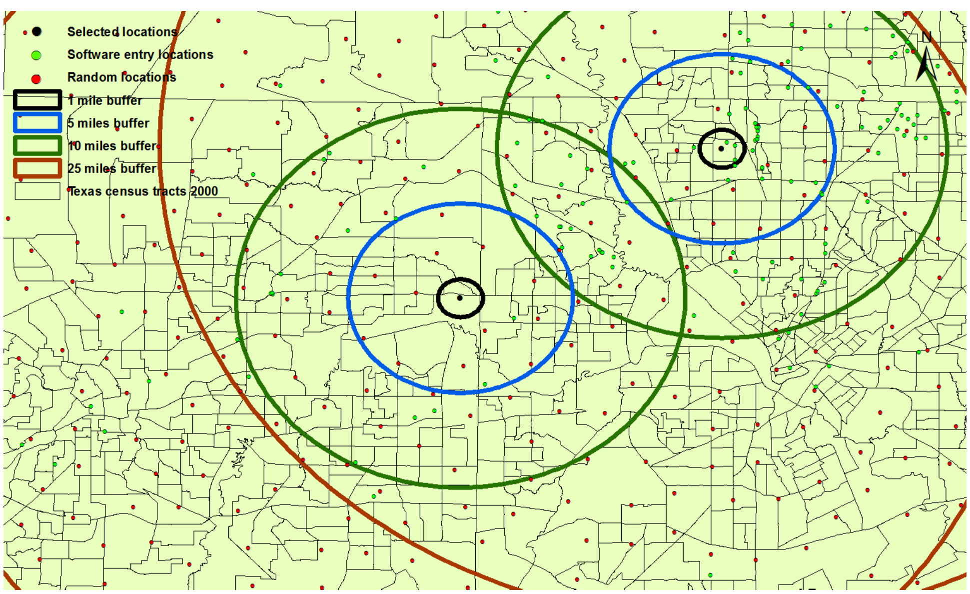


Panel B: 2006 Q4

Figure 5: Non-overlapping and selected locations in Dallas – Fort Worth



Panel A: Non-overlapping one mile rings



Panel B: Rings at two locations